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LECTURES
ON THE
THEORY AND PRACTICE OF MEDICINE;
NOW IN COURSE OF DELIVERY AT THE
THEATRE OF ANATOMY AND MEDICINE,
WEBB-STREET, SOUTHWARK.
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&c., &c.

EFFECTS OF LOSS OF BLOOD (concluded).—*The influence of various circumstances on the effects of loss of blood. The effects of loss of blood on the internal organs described: the brain, lungs, and intestinal canal. Treatment of the various effects of the loss of blood.* BLOODLETTING.—*Practical application of facts. Objections to the method answered. Mr. Wardrop's plagiarisms exposed.*

GENTLEMEN:—I shall now draw my observations on the effects of loss of blood to a close. We shall then be prepared for a more full discussion of the principles which should guide us in the institution of bloodletting, than that contained in the cursory notice of this subject in a former lecture. I proceed, then, to treat of—

THE INFLUENCE OF VARIOUS CIRCUMSTANCES ON THE EFFECTS OF LOSS OF BLOOD.

The first and principal circumstance which modifies the effects of loss of blood, is that which relates to the strength of the patient. *Cæteris paribus*, the degree of reaction is proportionate to the degree of strength. In infancy, in declining years, and in the feeble in constitution, there is defective reaction after loss of blood, the phenomena of which have been already detailed; the state of syncope is a state of danger; and a second or a third bloodletting is borne with difficulty. In youth, and in the vigorous and robust, on the contrary, the reaction is strong, and especially marked after repeated venesections.

In the strong, the state of sinking is ever preceded by that of great reaction, unless indeed, the strength be overwhelmed by the degree or early repetition of the evacuation; in the feeble it steals on insidiously and gradually, unmarked by reaction of the system.

The other circumstances which exert an influence on the effects of loss of blood, are certain states of disorder or disease. And I must, in this place, particularly remark, that the state of intestinal irritation is particularly apt to lead to those effects of bloodletting which I have described as exhaustion; while that of inflammation seems to protect the system from the effects of loss of blood: in the former case throbbing is soon induced, unless, indeed, it be prevented by a state more nearly allied to syncope; in the latter, bloodletting is followed by little of reaction until the state of inflammation be subdued, and the system be freely exposed to the uncontrolled influence of loss of blood: in the former there is danger from full depletion; in the latter, this measure is providentially not less safe than necessary.

Inflammation may, nay, in the course of events will, often supervene; but before this event takes place, the case is to be treated as different from inflammation. Bloodletting is not well borne. It may, if unwarily practised, even prove fatal. Whereas it is the remedy and the only remedy for inflammation.

The phenomena of reaction seem readily to assimilate with the constitutional effects of intestinal irritation, as is distinctly apparent in some of the cases already noticed; they are, on the contrary, to a certain degree incompatible with those of inflammation: so that, during the existence of acute and active inflammation, the symptoms of exhaustion are seldom observed; but they are very apt to display themselves immediately on the active state of inflammation being subdued; whereas exhaustion and intestinal irritation appear to co-operate in inducing a state of reaction in the system.

In all cases we are only to expect the phenomena of reaction when a certain quantity of blood has been lost; one bloodletting, although large, and even a continued drain, if not considerable, will not induce

exhaustion; the powers of the system are sufficiently great to recruit and to restrain its actions. But exhaustion is sooner induced under circumstances of intestinal irritation, and less so under those of inflammation, than in health; and reaction is the consequence, unless the strength of the patient be low, and then the reaction is defective, or even gives way to a state of positive sinking.

There are seldom the symptoms of reaction after one flow of blood, however great or profuse; it is the repetition or protraction of the cause, which, as I have already observed, is essential to produce this effect.

Each successive bloodletting is, of course, attended with increased risk. There is considerable danger where the reaction is strong; still greater when it is feeble. A large bloodletting in such cases may be followed by sudden death. There is greater danger when fainting has been several times induced, and when there is the least tendency to "want air."

ON THE EFFECTS OF LOSS OF BLOOD ON THE INTERNAL ORGANS.

We are altogether in want of a series of observations on the effects of loss of blood on the internal organs.

There is, I think, reason to suppose that a state of exhaustion from loss of blood, may lead to effusion into the ventricles of the brain; and a case published by the late Dr. Denman sufficiently proves that such a state of exhaustion is no protection against an attack of sanguineous apoplexy.

This patient had suffered from hæmorrhage from a fungous growth from the uterus, which many attempts were made to restrain by ligature, during the long interval of time between 1802 and 1809. Dr. Denman concludes this interesting case thus:—

"In February, and in May, 1809, two fruitless attempts were made to pass the ligature. Mr. Croft and myself then agreed to fix a small pair of denticulated forceps, not unlike those used in lithotomy, firmly on the stem, with the intention of occasioning the whole to decay, and of destroying, if it were possible, the regenerative power. But while we were preparing for this, the patient after passing the day, without any unusual complaint, went to bed in good spirits, but was found the next morning in a state of insensibility, with stertor. In this state she remained about twenty-four hours, and then expired. Leave was given to inspect the body, which, indeed, she had often expressed a wish might be done.

"The head was the part first examined. Nothing particular was observed about the membranes or surface of the brain; but in the ventricles were found about four ounces of blood, separated into coagula and serum. This extravasation was clearly the immediate cause of her death, little as it might have been expected, on account of the daily profuse discharge, to which she had for so many years been subject. It had often been remarked, that, although this patient was become much paler than she had formerly been, she was not comparatively thin in her person; and many who visited her, could hardly be convinced that she laboured under any dangerous disease."

There cannot be a more interesting case illustrative of the fact of a tendency to congestion of the brain, in circumstances of exhaustion from loss of blood, than that already quoted from Mr. Hey's work upon puerperal fever, attended by paralysis.

It is impossible, indeed, to imagine a more interesting and instructive train of events than those described by Mr. Hey. Nothing but a careful examination is wanting to make it complete as an illustration of the effects of extreme loss of blood upon the brain, lungs, and other organs of the body.

Mr. Travers mentions a case of pneumonia, in which a stroke of palsy, fatal the same evening, took place in the act of bloodletting. It is to be regretted that Mr. Travers does not mention what quantity of blood flowed before this event occurred; but as the case is given to illustrate the effects of loss of blood, it is to be presumed that it was considerable.

Paralysis has occurred in a state of exhaustion from other causes, as undue lactation; and in various circumstances of debility, as in cases of disorder of the general health, with sallowness and pallor, and a loaded tongue and breath. Of the former case Dr. Davis mentions an instance in his correspondence with Dr. Musgrave. Of the latter I witnessed an example in a very near relative. Amaurosis is not unfrequent in similar circumstances.

Effusion into the ventricles is doubtless a frequent effect of the extreme state of vascular exhaustion. The experiments of Dr. Seeds and Dr. Kellie confirm the fact.

After the brain it becomes necessary to examine the condition of the lungs under the influence of excessive loss of blood. I have already compared this state to that observed in the experiment of dividing the eighth pair of nerves, or otherwise subtracting the nervous energy. Still there is a difference; instead of hepatisation, with destruction of the texture of the lungs, there is œdema, with a clogged condition of the bronchia.

It is by the stethoscope, indeed, that the very first symptoms of sinking from loss of blood will be discovered, although a very early one is that rattle which I have already described as being audible without the immediate or mediate application of the ear.

The bronchia first become clogged, the lungs become œdematous, and the arterialisation of the blood is defective.

In two patients who died of hæmorrhage, examined by Dr. Hodgkin, the lungs were

found decidedly œdematous; in one, there was effusion into both cavities of the pleura and pericardium; in the other there were adhesions. In one the head was examined, and no morbid appearances found. A similar illustration of the effects of exhaustion from loss of blood, was detailed to me some years ago by Mr. Jowett, of Nottingham.

The state of flatulency, sometimes almost amounting to tympanitis, and the fœtid evacuations of the intestines, sufficiently denote the morbid condition of this internal organ.

There is, also, in extreme cases of exhaustion, a general tendency to serous effusion, both into the internal cavities and into the cellular membrane. This effect of the loss of blood has been very long remarked by medical writers.

As I have carefully avoided, Gentlemen, the statement of any circumstances which I did not think amply substantiated by well-observed facts, I shall leave this part of my subject to be elucidated by future observation.

I still have it in view to investigate the organic effects, and especially the remedies, of loss of blood by a series of experiments.

OF THE TREATMENT OF THE VARIOUS EFFECTS OF LOSS OF BLOOD.

The treatment of the effects of loss of blood must be resolved into that which is general or constitutional, and that which is local; and it must vary according to the peculiar state, or stage, of these effects. Syncope, reaction, and sinking, each require their appropriate treatment.

The constitutional treatment must be stimulant in syncope, sedative and soothing in the state of reaction, and restorative in that of sinking. The local treatment must vary with the organ chiefly affected, and with the mode in which it is affected.

When syncope assumes a dangerous form the principal remedies are, an attention to the posture of the patient, stimulants, and chiefly brandy, and the transfusion of blood The effect of posture is not, even now, fully known. It would be easy to allow the patient to lie over the edge of the bed, the head low upon the floor, and the feet greatly raised. In this manner such pressure would be restored to the encephalon as would, in many cases, support life, until other remedies being administered, the patient might be placed out of immediate danger.

I need not, in this place, notice the importance of a regulated mode of giving brandy and nourishment. I think it is frequently given in such quantities as actually to induce sickness, and its own rejection from the stomach, and so as to frustrate the object of the physician completely. The effect should be carefully watched. The physician ought not, of course, in such a case, to leave the patient for a moment.

The next remedy is transfusion. Unfortunately it has too frequently happened that the proper period of adopting this measure has been allowed to pass by. Not only the vascular system is exhausted, but, after a time, the functions of the nervous system have begun to fail. It might be a question, therefore, whether galvanism might not be usefully conjoined with transfusion.

It is an important point to determine how large a quantity of blood the system will bear to receive under various circumstances of exhaustion. Too much may overwhelm, too little may be inadequate to the accomplishment of the object in view.

It is, also, an important question whether the operation should be done at once, or at twice, or thrice, and with what intervals. As the system cannot bear a sudden reduction of the quantity of blood, so it may not be enabled to bear its too sudden restoration.

It is almost needless to add, that a due attention must be constantly paid to assist the arterialisation of the blood, by the admission of fresh air; and to sustain the animal heat by proper clothing, and especially warm applications to the feet.

If there should be convulsions, delirium, or coma, it may be necessary to apply a sinapism to the nape of the neck; and in the two former cases some mild sedative, as the tincture of hyoscyamus, may be of advantage.

In the case of excessive reaction, the remedies appear to be, first, extreme quiet of body and of the mind; then the mildest sedatives, especially the hyoscyamus; thirdly, the mildest nutriments; and, lastly, and above all, time.

The pain and throbbings in the head, the intolerance of noises, the general susceptibility to disturbance, the palpitations of the heart, alike demand the utmost quiet, to which everything soothing in the manner and treatment must be added. The tincture of hyoscyamus is, I think, the kindliest anodyne and sedative in these cases. The cause and other circumstances of the case point out the necessity for mild nutriment, to which, perhaps, the minutest quantities of brandy may be added.

It may be necessary to subdue the throbbing action of the head, by local bloodletting even; and it is most remarkable how small a quantity of blood being taken will relieve. Two or three leeches are frequently quite sufficient.

But the most unequivocal remedy is a cold spirituous lotion applied all over the head, by means of a cap consisting of one fold of stocking.

In exhaustion with delirium, the tincture of hyoscyamus should be conjoined, in full doses, with the other remedies. The morbid susceptibility, not only of the brain, but

of the heart, is greatly assuaged by this remedy.

In cases of exhaustion with sinking, stimulants must be administered abundantly. Cataplasms of mustard may be applied to the nape of the neck, and to the feet. It is difficult to imagine what would be the effect of the transfusion of blood; I have no doubt that galvanism would prolong life; and I should think the two remedies might be conjoined with advantage.

In all cases of exhaustion the functions of the bowels suffer. Constipation and flatulency are the usual consequences. These are best relieved by the warm water enema, which must, however, of course, be administered with due precaution, to prevent further exhaustion.

It is interesting to observe the blunted sensibilities in syncope and in sinking, and to compare them with the morbidly acute sensibilities of the state of reaction. Sinapisms to rouse, and the tincture of hyoscyamus to lull them, are, in their respective places, remedies of the greatest value.

I now proceed to treat of the subject of Bloodletting as the most important of our *remedies*.

It is one of the most remarkable facts in physic, that if several patients of similar strength and constitution, but affected by dissimilar diseases, be respectively placed in the erect position and bled to deliquium, they will be found to lose very various quantities of blood. I have known a patient, not apparently very feeble, faint on losing four ounces; and I have known patients bear to lose fifty, sixty, and even seventy ounces of blood without syncope.

This fact, plain and simple as it is, with its rationale and practical applications, has, I think, been greatly overlooked.

Its rationale is to be found, I believe, in connection with an equally interesting fact, that different diseases induce in the constitution different powers or susceptibilities in regard to the effects of loss of blood. Each disease appears, indeed, to possess its own peculiar and intrinsic virtue in this respect; this is determined by placing the patient perfectly erect, and bleeding to incipient syncope; the quantity of blood which flows is the measure of the protective influence of the disease, in one class of cases, and of its influence in superinducing a susceptibility to the effects of loss of blood in the other.

In cases in which it is doubtful whether the pain or other local affection be the effect of inflammation or of irritation, the question is immediately determined *by placing the patient upright and looking upwards, and bleeding to incipient syncope*; in inflammation, much blood flows; in irritation, very little. The violence of the disease, the powers of the system, and the due measure of the remedy, are determined at the same time. There is, in my opinion, no single fact in physic of equal importance and value in the diagnosis of acute diseases and in the use of a powerful remedy.

An interesting scale of diseases may be formed representing these properties. It would begin with congestion of the head, or *tendency to apoplexy; inflammation of the serous and synovial membranes*, and of the *parenchymatous substance* of various organs, would follow; and, lastly, *inflammation of the mucous membranes, of the skin, &c.* This part of the scale would be divided from the next by the condition of the system in health. Below this would be arranged *fever*, the effects of *intestinal irritation*, some cases of *delirium*, *reaction from loss of blood*, and disorders of the same class with *hysteria*, *dyspepsia*, *chlorosis*, and *cholera morbus*.

Persons in health and of moderate strength will generally faint, if bled in the erect posture, on taking fifteen ounces of blood. I have known seventy ounces to be taken in the sitting posture, in the tendency to apoplexy, without syncope; but the case is an extreme one. Patients with *meningitis*, *encephalitis*, *pleuritis*, or *pneumonia*, frequently lose thirty-five ounces of blood without fainting. In *bronchitis*, little more is borne to be lost than in health. A stout person in *fever* will frequently faint on losing ten, twelve, or fourteen ounces of blood. In *intestinal irritation*, with urgent symptoms even, the abstraction of nine or ten ounces of blood will generally induce deliquium. In *delirium tremens*, or *puerperal delirium*, the patient soon faints from loss of blood. The same thing is still more observed in those cases of violent reaction which arise from loss of blood itself. In *dyspepsia*, *hysteria*, and *chlorosis*, the susceptibility to syncope from loss of blood is very great; and I have known a patient of good strength, affected with *cholera*, faint on taking four ounces of blood, who had shortly before borne to lose nearly twenty ounces without faintishness, under the influence of *inflamed mamma*. As cases of the diminished tolerance of loss of blood, I would also enumerate paralysis from *laceration of the brain*, and apoplexy from *concussion*, before reaction or inflammation be established. It must also be carefully noticed that cases of accident—that of fractured ribs, for example—do not bear the loss of blood like those of inflammation. Surgeons are very apt to err in such cases.

The practical application of these facts consists chiefly in their affording—

1, *A Rule for bloodletting, in all cases in which this measure is required to be fully instituted.*

2, *A Guard at once against inefficient and undue bloodletting*; and

3, *A source of Diagnosis, in the fullest sense of this word.*

The rule is suited also to the *degree* and the *duration* of the disease, for with each of these its influence in inducing tolerance or intolerance of loss of blood is respectively augmented.

It is not less adapted to those most frequent of all events, *mixed* cases. Inflammation and irritation may be conjoined; for example, there may be mere nephralgia, or absolute nephritis, from calculus, or a mixed case involving both. There may be mingled intestinal irritation and inflammation. In each of these circumstances, the rule for bloodletting which I have proposed, adapts itself accurately to the demands of those various morbid affections, and to the actual strength and condition of the general system.

It is difficult to say whether more injury has been done by an undue or by an inefficient use of the lancet. In inflammation we must bleed fully. In irritation we must bleed cautiously. Inefficient bloodletting, in the former disease, and undue bloodletting in the latter, are alike dangerous or even fatal to the patient; from both extremes we are guarded by the rule which I propose. By directing the patient to be placed in the erect position, and bled to incipient deliquium, we shall often take much more blood than we should have ventured to *prescribe* to be taken in inflammation, and very much less than we might be disposed to direct, in irritation; and in both these cases the rule conducts us to the only safe mode of treatment.

And if much blood has flowed before syncope has occurred, we must suspect inflammation; if little, we must suspect that, however similar the symptoms, the case is in fact of a different nature—perhaps irritation, perhaps exhaustion.

I have also found that, in every case in which early syncope occurs from bloodletting, the more remote effects of loss of blood, as reaction, or sinking, are also very liable to occur; and it is in these cases that sudden dissolution has followed the use of the lancet. There is, in every point of view, intolerance of loss of blood. The reverse of all this obtains in inflammation, which seems to be incompatible, to a certain degree, with the effects of loss of blood; these are, on the other hand, very apt to supervene as the inflammatory action subsides.

And here I would solicit the co-operation of the profession in the further investigation of this important subject. It is by the multitude of facts alone, that the propositions which have been stated can be established or corrected. With the view of obtaining these facts I would propose that, in every case in which full bloodletting is to be instituted, the patient be placed perfectly erect in a chair, or in bed, and bled to the very first appearance of deliquium; the quantity of blood taken is then to be noted, and accurately registered in a table. The same thing is to be observed on each repetition of the bloodletting.

It is obvious that none but the most unequivocal cases should be thus registered. Cases, the diagnosis of which was not perfectly clear, would only add their own obscurity to the investigation.

It is equally obvious that the investigation proposed cannot fail to add useful facts, which will, in their turn, become useful guides to the physician. It is still true, as Celsus has observed, "Nulla perpetua præcepta medicina recipit." To the young practitioner, however, I think the practice proposed will prove of great assistance; and if it preserve one from the bitter reflection, which too many have experienced, of having done too much or too little, I shall not esteem that my exertions in introducing it have been in vain.

I would observe, in conclusion, that I do not think it safe, in any case, to bleed to actual deliquium, in the recumbent posture. But there are few cases, if any, in which, if it be proper to bleed fully at all, danger can accrue from bleeding to the most incipient syncope, in the perfectly upright position. Besides, the remedy is at hand; it consists simply in laying the patient recumbent, and, if necessary, raising the feet and depressing the head.

It may become a question, whether the patient may, in a little time, be again placed erect, so as to reproduce a state of slight deliquium, and thus to add to the power of the previous bloodletting in subduing the disease. But I do not think a state of continued syncope free from danger. I have known it lead to delirium, or apparent sinking.

On the other hand, the influence of an opposite position, the head being placed extremely low, and the lower part of the body being very much raised, has not been sufficiently traced, in the various cases of the immediate or remoter effects of loss of blood.

Amongst the other objects of this "proposal," is that of collecting any modifications or exceptions, in regard to the rule which I have laid down. It cannot be imagined that it should be without exceptions. It is as important that these should be pointed out, as that the rule itself should be established. There are already two exceptions to the rule which I have proposed, which I would briefly mention. In some cases of fever requiring bloodletting, the patient cannot support the erect position; in such a case, the arm must be first prepared; the vein should then be promptly opened; and then the patient should be gently raised and supported in the upright position, carefully avoiding all muscular effort. On the other hand, in the case of congestion of the brain from exhaustion, there is not such early syncope from blood-

letting as might be expected; and yet it is obvious that the system cannot bear the loss of blood. I have known this to obtain in exhaustion from undue lactation.

It will also be an interesting question whether this rule, in its repetitions, besides excluding undue bloodletting on the one one hand, and inefficient bloodletting on the other, does not secure the cure of the disease, with the least possible expenditure of the vital fluid.

It seems almost needless to allude to the case of early syncope from *timidity*. It is only necessary, to arrest the flow of blood, to lay the patient recumbent, and to wait until his timidity has subsided.

Two objections have been made in reference to this rule for the administration of bloodletting; the *first* is, that, in some cases not inflammatory, more blood might be taken than the patient could bear to lose, in order to institute the test; my reply is, that such cases are not included in my proposition, which only relates to cases in which bloodletting "is required to be *fully* instituted." The *second* is, that, in some cases, more blood ought to be taken than would flow before syncope is induced. I greatly doubt this assertion; I think it replete with peril; but, if it be true, let the patient be replaced in the recumbent posture,—wait a few minutes, and again let the blood flow; we have at least ascertained the state of tolerance of loss of blood, and this fact will guide us in determining how much blood may be withdrawn; it is a fact added; it is knowledge substituted for what must otherwise be ignorance.

I have known persons speak boldly of taking twenty ounces of blood in fever, who would be struck with terror at the idea of withdrawing forty or fifty in apoplexy from congestion; and yet the former is a dangerous, the latter a safe, measure, comparatively. Both, however, must be regulated by the plan which I have proposed, so as to be free from every kind of danger.

These remarks, Gentlemen, were first published (in 1826); they have since been fully and explicitly confirmed by Mr. Wardrop, in lectures published in THE LANCET (1834), and separately (1835) in similar words:—

"In the last lecture I endeavoured to impress on your minds more particularly three points: 1st. The peculiarly incompressible state of the pulse, which, in conjunction with other symptoms, indicates the propriety of general bloodletting; 2nd. The importance of the first bleeding, and the almost insuperable difficulties which are to be often overcome when this first depletion has been too sparing; and, lastly, I endeavoured to point out the propriety of abstracting blood until a fainting state of syncope supervened, in all cases wherein venesection is decidedly preferable to local bleeding.

"You are now naturally led to inquire—What is this quantity of blood which it is necessary to abstract at the first bleeding, in order to produce a state of fainting? Such, indeed, is the variety of the constitution of individuals,—such the difference in the severity of disease,—and such the difference of the period when called on to treat particular cases, that you will find, that whilst in some the pulse sinks after the removal of but a few ounces of blood, in others depletion must be carried to a great extent before syncope is produced.

"When employing venesection, and observing the indications which have already been pointed out in order to regulate its extent, it will be generally found that the quantity of blood which you are able to remove before fainting comes on, is, in fact, never more than is requisite for the cure of the disease. Hence a person in health usually faints from the loss of a comparatively small quantity of blood, whilst the same individual, after suffering but a few hours from active inflammation, requires to lose an almost incredible quantity,—a quantity essential for the cure of the disease,—before he falls into a state of syncope."—"A large quantity of blood may be taken away at the first bleeding before syncope is produced, whilst, at every succeeding operation, fainting comes on from the loss of a smaller and a smaller quantity.

"If you are to be guided in the employment of bloodletting by the principles which I have been endeavouring to inculcate, you will readily conceive the differences in the quantities of blood which must be abstracted in different examples even of the same disease; and whilst, in some instances, you will be disappointed, from the smallness of the quantity which flows from the vein before syncope supervenes, you will in others be surprised at the extent to which it may be carried, and with the happiest results. Hence the difficulty of attempting to give anything like a precise idea of the quantity of blood which patients require to lose in the treatment of particular diseases.

"The state of fainting is to be considered as an index of the quantity of blood necessary to be removed for relief from the disease; and, as I have already said, you will always find that quantity to be in the ratio of the propriety and necessity of procuring it.

"There is a class of cases in which bloodletting is often employed unnecessarily, and often too, perniciously. I allude to the common practice of bleeding persons immediately after an accident, or during an apoplectic or convulsive fit. In many accidents, more particularly where the head suffers, the first effects of the injury are a diminution or collapse of the vital powers; and if, under such circumstances, bloodletting be had recourse to, a still further

diminution of the vital powers is produced. It is not until the powers of life have revived, or that a reaction has taken place, that you should, after severe injuries employ blood-letting, and even then it ought to be had recourse to with great circumspection."

"In proportion to the violence of an apoplectic shock, so are the powers of life diminished: and hence, if the quantity of blood abstracted be regulated by the severity of the symptoms, in like proportion will it be hurtful by still further diminishing the vital powers. When a person is in a state of insensibility from an apoplectic fit, those around are too apt to urge the necessity of bleeding, conceiving that the loss of blood will cure the disease in the head, of which the fit is merely an effect or symptom."

"There are cases of plethora or congestion in the brain, producing a sudden loss of the intellectual powers and convulsions, in which too much blood can scarcely be removed to save life; but in such cases the pulse is strong, usually acquiring vigour whilst the blood is flowing from the vein."

"In cases where there are organic changes in the brain's structure, and when the sudden apoplectic attack is caused by some vessel of the diseased part giving way and pouring out blood, bloodletting is of no avail, and when had recourse to when the pulse is feeble, and the vital powers already are much diminished, it never fails to hasten the patient's death: therefore, in such cases, bloodletting ought to be resorted to with great caution."

It is not a little interesting to know that similar observations and similar inferences obtain in veterinary practice. For a knowledge of this fact I am indebted to Mr. John Field, jun., of Oxford-street.

In concluding this brief notice of bloodletting as a diagnostic, &c., I must adduce one of the most interesting cases I ever witnessed, and one of the most important in its results:—A gentleman, residing twelve miles from London, was taken with an affection of the head. I was sent for, and found on inquiry that he had been bled early, and that he had speedily fainted under the influence of loss of blood. The case was long one of great anxiety. Another physician was consulted. It was supposed to be "ramollissement." Confiding in the diagnosis afforded by the *early* syncope, in the *early* stage of the disease, from bloodletting, I continued to hold out hope of recovery. The patient did recover. Now, the hope I felt and held out, in this case, flowed almost entirely from the confidence I reposed in the diagnostic afforded by the event of the bloodletting.

A short time ago I was consulted by a medical friend in the Edgeware-road in the case of his wife; he feared peritonitis; there had been previous attacks of the same kind; there were great pain and tenderness of the abdomen; the tongue was loaded. I prescribed bloodletting in the upright position; the patient, though young and stout, fainted on losing eight ounces of blood. From this moment there was no obscurity in the diagnosis, no hesitation in reference to the treatment, no interruption in the recovery.

There is a final observation relative to the use of bloodletting; in all cases in which there is great tolerance of blood, it is *remedial* and *safe;* in all cases in which there is intolerance, it is proportionately of *doubtful efficacy*, and *replete with danger*. How long will the profession neglect a source of such valuable information? How long shall we have to lament the opposition of persons, lecturers too, who have never understood, never tried its value?—an opposition so feeble as not to deserve a serious refutation.

LECTURES
ON
MATERIA MEDICA AND THERAPEUTICS;
NOW IN COURSE OF DELIVERY
AT THE
WINDMILL-STREET SCHOOL OF MEDICINE
BY
GEORGE G. SIGMOND, M.D.

CASCARILLA.—*Its medicinal powers. Properties of the euphorbiaceæ. Preparations of the cascarilla bark contained in the Pharmacopœia.* ULMUS CAMPESTRIS.—*Its use in medicine. Description of the tree and bark. Effects produced by the bark on the economy.* ILEX AQUIFOLIUM.—*Discovery of ilicine. Analysis of the bark; forms in which it may be administered.* QUERCUS PEDUNCULATA.—*General history of the oak tree. The bark and acorn. Discovery of tannin.* QUERCUS INFECTORIA.—*Mode of production of the galls. Preparations in use; value of the gall-ointment in cases of piles.*

GENTLEMEN:—Although the bark commonly known to us under the name of cascarilla, can never be considered as a substitute for cinchona, yet it may be employed with considerable success in fevers, and in diseases of debility. The uses to which it was, when first introduced, generally applied, were to give an agreeable aroma to tobacco during smoking, and to produce some degree of intoxication when mixed with the herb. Stisser, Professor in the *University of Juliers*, published a book in the year 1693, in which he states that some of it had been given to him by an Englishman of some celebrity, and that he had

made tinctures of it, and tried its medicinal powers in calculous affections, in asthma, phthisis, scurvy; that he had reason to be satisfied with his success, and adjudged it to be diuretic and carminative. It was afterwards sold for cinchona bark, and the cinchona lancifolia was called cascarilla; indeed, it seems that this name was indiscriminately given by the Spaniards to any bark that was in small pieces, and of a delicate texture.

It has been supposed that it is the clutia eleutheria of Linnæus, and not the tree, or shrub, to which he has given the name of croton cascarilla, by which the London Pharmacopœia recognises it, that furnishes us with the bark. It is from the Bahama Islands that we are principally supplied with this drug. Dr. Woodville says, that among other circumstances which tended to involve the parental source of cascarilla long in uncertainty, was the assertion of some authors that it was a native of the Spanish main, and was thence imported into Europe; thus founding a presumption that the cascarilla and eleutheria barks were different, and the latter only was the produce of the Bahama Islands: but this assertion we have discovered to be contrary to the fact; for, upon inquiry, we do not find that this drug was ever imported from Spanish America, but that the Bahamas have constantly supplied the European markets with cascarilla bark, a parcel of which was sent here from one of those islands, along with specimens of the tree producing it; but it is necessary to observe here, that Dr. Wright, in his account of the medicinal plants growing in Jamaica, gives the name of croton eleutheria to a tree, the bark of which, he says, is the same as the cascarilla, or eleutheria, of the shops. The croton cascarilla belongs to the euphorbiaceæ tribe.

An interesting monograph of Adrian de Jussieu gives us the most correct information we possess of the general properties of this large group, and most singularly do they vary; the prevailing character is certainly acrid; many of them are poisonous; amongst these the celebrated manchineel, the hipponane mancinella, is said to be so destructive, that those who have slept under it have perished, whilst the seeds of the ricinus communis, the jatropha curcas, and others, are very active in their agency upon the intestinal canal. There are, however, some families that possess aromatic properties, and the barks are found febrifuge, such as the croton cascarilla, the bridelia spinosa, and some others.

The species called the cascarilla bark tree is described as rising about twenty feet in height, and sending off numerous branches, especially towards the top. It is said that the external appearance of the bark varies on the branches from that which is observed on the trunk, for that on the former it is brown and smooth, that upon the latter it is of a much whiter colour, and very rough. In the greater number of the euphorbiaceæ there is a milky secretion, in which resides the peculiar principle by which each family is recognised. From the cascarilla there is an exudation from the branches, when they are broken, of a thick balsamic juice, but its properties have not been examined; the leaves are entire, ovate, lanceolate, somewhat cordate, and elongated towards the apex; they are on short petioles; the upper surface is studded with small orbicular scales, and is of a bright green colour, whilst the under surface is whitish, shining, and silvery.

The flowers are in axillary and terminal spikes, and are composed of a calyx, divided into five ovate leaflets; the petals are whitish, obtuse, oblong, and spreading; the small flower has ten subulate filaments, bearing erect compressed anthers; the female produces a roundish germen, supporting three bifid spreading styles, with obtuse stigmas. The capsule is oblong, marked with six furrows, and divided into three cells, each containing a solitary oval shining seed. It is imported into this country in large bales, or chests. It is found in quills, about six or eight inches in length, and about the tenth of an inch in thickness. It is covered externally with a whitish epidermis, rough, marked with lines, and covered with a great variety of lichens, of which Fée has furnished us with a catalogue, consisting of some of the opegrapha, graphis, glyphis, fissurina, arthonia, sarcographa, chiodecton, trypethelium, parmentaria, pyrenula, porina, verrucaria, thelotrema, coniocarpon, cecidea, and parmelia. On being tasted it gives a warmth to the mouth, a bitterness, and an agreeable aromatic flavour also, which remains for some time. Its natural odour is aromatic, but not particularly striking; when it is burnt, however, it gives out a powerful and very agreeable effluvium; it easily takes fire, and is quickly consumed; it has been used as a perfume when thus burnt. It is, without much difficulty, reduced to powder, which is of a greyish-brown colour. It yields up its properties to water and alcohol. We have an authorised infusion, made of an ounce and a half of bruised cascarilla bark, macerated for two hours in a pint of boiling distilled water, in a vessel lightly covered, and strained; of this preparation an ounce, or two ounces, is the usual dose. We have, likewise, a tincture, thus directed to be made:—Take of cascarilla, bruised, five ounces; of proof spirit, two pints; macerate for fourteen days, and strain; of this a drachm, or two, is usually prescribed.

In the Dublin Pharmacopœia there exists a recipe for a resinous extract of cascarilla, but it is not often ordered in that form; besides which it is expensive. An ethereal

tincture has been occasionally used, but the infusion and the tincture of our pharmacopœias appear amply to supply us with the means of using the cascarilla when it may be required. The tincture is more generally preferred, and is very often united with decoction of cinchona, to which, if it does not impart any additional efficacy, it gives a fragrancy and pungency which very much assist the stomach in bearing the Peruvian bark. The powder is used from half a scruple to a drachm, being the minimum and maximum of its dose.

As I shall give a general view of the barks and the roots, I shall not now discuss the general merits of this drug, but state to you that some particular virtues have been assigned to it. It has been stated, and I have reason to believe the fact, that it has some influence in restoring the appearance of the menstrual excretion, after it has been interrupted by fever. It has a marked influence if the suppression has occurred from intermittent fever, or even from catarrhal fever; but it is not to be considered on that account as an emmenagogue; its power is only to restore periodicity, under peculiar circumstances; and this has, also, been remarked where hæmorrhoidal flux has been interrupted by fever. It was Louis Appinus, of Hersprnch, who first, in the year 1694, pointed out this singular property of cascarilla. In an epidemic dysentery, in 1719, in France, this remedy was found more efficacious than any other. Dr. Underwood had a very high opinion, founded upon experience, of the remedial effects of this bark in the gangrenous aphthæ, which sometimes proves so fatal to children. In diarrhœa it is sometimes found useful, where it is dependent upon loss of tone of the intestinal canal.

THE BARK OF THE COMMON ELM.—ULMUS CAMPESTRIS.

I do not think that the remedial powers of the bark of the common elm have been sufficiently appreciated in this country. I have found it so serviceable in some diseases of the skin, so useful as a tonic, and valuable as what is called an alterative (a term I shall hereafter explain), that I thought it right to recal the attention of the profession to it, in a paper which I sometime since read before the *Medico-Botanical Society*. Since the days of Dr. Lettsom, it seems to have been consigned to an undeserved oblivion. I think, however, that it may, under some circumstances, be employed, when the more expensive barks have little efficacy; and I think that, with the exception of the Peruvian bark, we have no tonic more generally to be employed, and when we know the facility, and of course the cheapness, with which it is to be procured, I am surprised that it has been so little investigated. The common elm appears to be indigenous to this island, which is almost proved by the number of places named after it, which are to be found in our oldest records: no less than forty places are to be found in "Doomsday-book," which are called after it. It grows with great luxuriance in many parts of the kingdom, some nearly three yards in circumference near the root, and is of great longevity. Some are known which tradition declares to have stood nearly three centuries. Elms are, for the most part, large handsome trees; though, properly speaking, it is not a forest tree, none of the brethren of the forest surpasses it in its grandeur and beauty. They are highly picturesque, and their wood, though not so valuable as oak timber or pine, is of singular use as timber, for whether exposed to the action of water, or to excessive dryness, it changes but little; hence its utility in water-works, pumps, aqueducts, and ship-planks; it also makes the second best kind of charcoal; its leaves are often laid by as a substitute for fodder, where it is scarce; and horses, cows, sheep, and swine very gladly eat them. These formed a very large proportion of what was sold, some time since, under the name of the "British herb tea," and are said to make an agreeable and refreshing beverage when infused for about ten minutes in warm water. It is not very long since that five-and-forty hogsheads of these dried leaves were seized, and condemned by the officers of the Excise to be burnt as imitation tea. The inner bark of the elm is very nutritive, and in some countries, especially in Norway, it is dried and ground to powder, and mixed with meal to make a coarse kind of bread. The flowers have a violet smell. The bark of the young trees and the boughs of the old are smooth and very tough, and can be peeled from the wood a very considerable length without breaking; but when older, it is very quickly rent. The inner tough bark, which is the part medicinally used, has no remarkable smell, but a bitterish taste, and abounds with a slimy juice; it is of a yellowish colour when it becomes dry; it is very tough and hard to cleave. It is brought to the market after the epidermis has been removed. It is found that the bark which is taken from the branches, has a much more bitter taste than that which is stripped from the trunk. There are several varieties of the elm, and it is found that they may all be increased by grafting on the broad-leaved wych elm. These may be raised from seed, and will be of a proper size to receive the graft when they have been two or three years in the nursery. The time for peeling has been differently stated; that, however, which is taken from the tree about May, has most generally succeeded best as a medicine.

In the year 1797, Vauquelin published a paper entitled, "Observations upon a Malady to which Trees are subjected, and

specially attacks the Elm, and which is analagous to an Ulcer." In this paper he describes and analyses two kinds of morbid matter which flowed from the ulmus campestris; the one whitish, and nearly as limpid as water; the other of a thicker consistency, and coating the bark of the elm with a sort of varnish. In 1804, Klaproth published his chemical experiments on a gummy juice from the stem of an elm. Dr. Thomas Thomson pursued this investigation further, and gave to this vegetable exudation the name of ulmin. Berzelius has since found that this is a vegetable substance not confined to the genus ulmus, but that it forms a constituent part of almost all trees. He found it in the bark of the cinchona officinalis, and in that of the pinus sylvestris. Ulmin is nearly tasteless, dissolves very slowly and in small quantities, both in alcohol and in water. The solution is pale-brown, and tasteless; its most striking property is its affinity for carbonate of potash; it is by means of carbonate of potash that it is usually soluble in water. It has as yet not been applied to any useful purpose; but it has been stated that the bark should be stripped from the tree when this exudation commences, or that the consequence is an alteration in the constitution of the principles. The decoction of the elm bark of the Pharmacopœia is thus made:—Take of fresh elm bark, bruised, two ounces and a half, distilled water two pints; boil down to a pint and strain. This decoction is of a brownish colour; it is slightly mucilaginous; it has a faint odour; a taste somewhat bitter, but by no means disagreeable. The stomach will bear a very large quantity, two or three, or even four, ounces at a time, without the slightest inconvenience. The tincture of cardamoms, or of cascarilla, about a drachm to two ounces, may be added, but not a larger quantity, for alcohol causes a precipitation to take place in the form of light-brown flakes, and hence there appears to be some reason to suppose that an alteration may take place which might materially influence its therapeutic agency.

The patient, after the quantity I have just mentioned, feels rather an agreeable glow of heat at the surface of the body, and soon a gentle perspiration occurs, the quantity of the urinary secretion is increased, and the water is generally more limpid; if the medicine be persevered in the appetite is increased, the bowels seldom exhibit any result, but both the urine and the perspiration are increased. The action of the heart seems to be gently increased, for the pulse becomes somewhat quicker, the smaller capillary vessels are often red with blood, hence the cheek occasionally becomes flushed, and there is a general warmth diffused over the system. I know of no bad effects which have been at any time the result of its administration, although it has been persevered in for a considerable length of time. It was at a very early period supposed to act upon the skin, and Quincy, in his "Dispensatory," says, "Some have supposed it to possess great virtues in curing cutaneous foulnesses." Dr. Lettsom found it most effectual in a case of lepra which had resisted all the medicines usually employed; it was of that character to which the term icthyosis has been applied. Burnett observes, that this disease is seldom submitted, in this country, to medical treatment, as the "scaly women" and fish-skin may earn a livelihood by exhibiting themselves at fairs and wakes, and such a disease is by them considered a personal estate of the value of from two to three hundred pounds per annum, which they would be very unwilling to part with, and are only too happy when they can entail it upon their children. I shall have occasion to dilate on its great utility in diseases of the skin at another opportunity.

THE HOLLY, ILEX AQUIFOLIUM, AND ILICINE.

M. Rousseau obtained from the *Medico-Botanical Society* of London a silver medal for the important information he gave relative to the uses of the holly, and the principle in which its medicinal virtues reside, and the ilicine is now received into the French Pharmacopœia and used as a substitute for cinchona. In the "Transactions" of the Society will be found a translation of the original communication.

Those authors who have spoken of the holly consider it as possessing emollient, resolving, diuretic, and expectorant properties; few consider it as a febrifuge. Before it was employed internally many remarked that the bruised and putrified bark yielded a viscous substance called birdlime, and which was applied externally as a cataplasm. M. Chomel employed it to alleviate the pain of gout.

The root, according to many, has the same emollient and resolving properties as the bark. The decoction of the leaves, or their powder, has been praised in catarrh, pleurisy, colic, and the diseases of the urinary organs. Haller recommended the juice of the leaves in icterus.

We are indebted to M. Lassaigne, a zealous friend of science, for the first analysis by which he undertook to discover the composition of the leaves of the holly. This analysis furnished him, viz.,—1, wax; 2, chlorophylle, (green matter of the leaves); 3, a bitter matter, which is neutral, non-crystallisable, soluble in alcohol, and undecomposable by the acids and alkalies; 4, a yellow colouring matter; 5, gum; 6, acetate of potass; 7, muriate of potass and lime; 8, supermalate of lime; 9, sulphate and phosphate of lime; 10, fibre of the wood (lignin).

The leaves of the holly may be administered in different ways: in decoction, in sub-

stance, as bitter principle called ilicine, and in enemata.

1. The *decoction* is prepared by boiling half an ounce of the freshly-gathered or dried leaves in eight or ten ounces of water, which is to be boiled down to one half. This quantity is to be given in one dose two hours before the paroxysm, and to be continued for eight or fifteen days if the fever does not yield at the fourth or sixth dose.

2. In *substance*, the leaves are to be dried and reduced to powder, and passed through a seive, and one or two drachms macerated in ordinary white wine for twelve hours, or made into a decoction with water without being strained. This dose should be given two or three hours before the paroxysm.

Ilicine.—This febrifuge principle, in which the bitter matter of the plant resides, should be administered in doses of six, twelve, eighteen, or twenty-four grains, in the form of pills, as least disagreeable to the patient, and the use of it continued until the disease is entirely subdued.

THE OAK TREE, QUERCUS PEDUNCULATA.

To every member of civilised society, the history of the oak, whether natural or general, must afford peculiar interest, it is almost interwoven with the annals of mankind. In this country, from the earliest period, it has been invested with the most reverential regard. The religion of the Druids, and the superstitions connected with it, taught our forefathers to look upon it as a hallowed tree; and, as the bulwark of England, the British naval oak has arrested the attention of succeeding Governments, and of a people who derive from it such inestimable advantages. The naturalist and the physician have inquired into its botanical and medical uses, and though they may not have obtained from it such sources of relief in the cure of disease as they have derived from less remarkable objects of cultivation, still it is not altogether to be neglected, or passed by in silence.

Of the family of the oak there are now known about one hundred and fifty species; the greater number of these are natives of America. Our knowledge upon this subject has very much increased since the days of Linnæus, who only enumerates fourteen species; Wildenow, in 1806, describes seventy-six. In North America, two naturalists, father and son, discovered twenty-six, whilst Humboldt and Bonpland observed, in South America, twenty-four. We have, in this country, two distinct species, the quercus robur and the quercus sessiflora; the former is the quercus pedunculata of Wildenow, and the quercus femina of Withering; the other species to which I have to call your attention, is the quercus infectoria, a native of Asia, which gives to us the nut-galls of commerce. The quercus robur of British oak, is remarkable for the slowness of its growth, for its strength and its longevity. For a very curious and interesting collection of facts upon this subject, I must refer you to a very valuable memoir from the pen of my late friend, Professor Burnett, entitled, "Amænitates Quernæ." In forests, the tree rises to a very considerable height, but when it grows alone it is rather a spreading tree. The leaves are oblong, smooth, sinuate, nearly sesile, or on very short footstalks, their upper surface of a rich shining green, but paler underneath. The male flowers are in numerous pendulous, downy calkins; the females on axillary simple stalks. The calyx of the male flower is a scale of one leaf, bell-shaped, and, generally, five cleft; that of the female is double, the outer coriaceous, becoming larger, and forming the hard woody cup of the acorn, the inner, of one leaf divided into six segments surrounding the base of the germen. The filaments are ten in number; the germen is ovate, crowned with a short conical style, and three stigmas.

It has been very properly observed by Du Hamel, that oaks in forests, being propagated from the acorn, assume so many varieties that it is difficult to find two resembling each other in every respect. There are also many varieties of oak which are distinguishable by their qualities, the uses to which they are directed, and by various accidental circumstances, from which they acquire peculiar names, which, as they are merely local, and are not based upon any permanent characteristics, cannot be fully ascertained. The acorn has, from an early period, been prized as an article of food for swine; and the value of woods, in the days of William the Conqueror, was estimated, as we learn from "Doomsday Book," by the number of hogs they would fatten. In the Saxon laws, some are to be found made by King Ina, which relate to the fattening of swine in woods, and which have come under the name of pawnage, or pannage. As an article of food the acorn has not been used in this country, but we learn that, in the days of Pliny, it was brought to table during the dessert in Spain, and to this day the custom continues. Cervantes describes Don Quixote as selecting the choicest for a lady at the goatherds dinner. In some parts of the South of Europe, it is now occasionally, when famine threatens, used as food. It has been employed medicinally, and acts as an astringent upon the bowels; it has been roasted and powdered, and given, with good effect, in diarrhœa and in dysentery, but I do not know that it has ever been received into any of the Pharmacopœias; it has been also used to check hæmorrhage externally when thus prepared, and some of the rural classes have much faith in its styptic powers, which have been traditionally handed down from time immemorial.

The bark of the oak is the only part that has been generally acknowledged as medicinal, and has, as such, been recognised by the different authorities, and a decoction is to be found in the London Pharmacopœia thus formed:—Take of oak bark ten drachms, distilled water two pints; boil down to a pint, and strain. It is to a peculiar proximate principle, called from its being employed in tanning, tannin, that the astringency of the oak and of other barks has been ascribed; besides which, gallic acid, and another acid to which the name of tannic acid has been applied, are discoverable. According to Sir Humphrey Davy's experiments, an ounce of oak bark afforded one hundred and eleven grains of solid matter by lixidiation, of which seventy-seven were tannin, and this quantity was obtained from the inner cortical part, whilst the cellular integument gives only forty-three grains of solid matter, of which nineteen only are tannin; but the proportions vary very much according to the age of the tree, and the season at which the bark has been stripped from the tree. In the spring it appears that there will be four times as much tannin as at the autumnal season of the year.

About ten years ago Dr. Dodd read before one of the medical societies a paper on the exemption of tanners from phthisis pulmonalis, and the efficacy of the aroma of oak bark in the cure of that formidable disease. He had been led to examine this subject from the case of a patient, a weaver, who had all the symptoms of phthisis pulmonalis, for which he had prescribed, not with a view of curing, but of mitigating; he, however, lost sight of his patient, finding that he had suddenly quitted his residence. Twelve months afterwards he met the man a strong, stout, and healthy person; he learnt that the man had given up his former occupation, and taken to that of tanning, assigning, as a reason, that tanners were never affected with consumption. Dr. Dodd went very quickly to Bermondsey, where there are collected more tanners than in any other place. One resident practitioner testified that for thirty years he could not remember a death from this cause; and another gave the same account during twenty years. He found that there were seven hundred tanners employed at Bermondsey; that the deaths were about the usual average, but that none were to be attributed to consumption. He further found that those who lived in the immediate neighbourhood of the tan-yards were exempt likewise, although the situations were low and damp; and this he attributed to the aroma which, besides its pungency, he believed to be possessed of some antiseptic power. He had been led to try the aroma, and invented an apparatus by which the imbibition of the aroma might be facilitated. The subject excited much attention at the time, but it was generally received that little influence upon disease has been the result of the aroma; and it has been ascertained that although the tanners are, generally speaking, healthy, yet they are by no means remarkable for longevity. The idea that the aroma of different barks is highly beneficial in disease of the lungs has for some time been prevalent, but the experience of medical men has proved it to be quite fallacious. There are many instances on record in which females, in the very earliest stages of phthisis, have been directed to wear a chemise quilted with bark, without deriving from it the slightest advantage. A very expensive mode of cure has been suggested, which I should think likely to prove quite as inefficacious; viz., the burning of cinchona bark, and also the other barks; although, certainly, very considerable relief has been momentarily afforded where the expectoration from the lungs has been very considerable, by burning, in close rooms pastiles which have been principally formed of the charcoal of the cascarilla bark; this, however, seems to be as much dependent on the heated state of the atmospheric air, as upon any aroma arising from the heated wood.

Some persons were sanguine enough, at one period, to expect as much relief from a decoction of oak bark, in the cure of intermittent fever, as from the Peruvian bark. This idea has been long abandoned. It is now very rarely used; still, however, it has some claims to consideration as an astringent; it will restrain alvine fluxes and mucous evacuations, and has been sometimes known to be of use in hæmoptysis, and also in menorrhagia. It makes an excellent astringent gargle, in the form of decoction with alum, when there is slight inflammation of the mucous membrane lining the throat. Dr. Cullen had much faith in its powers in such cases, and Dr. Underwood has very strongly recommended it in the coryza of infants. It is useful as an injection in prolapsus uteri, in leucorrhœa, and also as a fomentation in some states of hæmorrhoids.

QUERCUS INFECTORIA.—ORIENTAL GALL OAK TREE.

This oak is spread throughout Asia Minor; it has rather the habit and appearance of a shrub than a tree, rarely attaining as much as six feet in height. It is to Olivier that we owe our knowledge of the circumstances under which our nut-galls are obtained, and of the tree on which they are produced. Previous to his elucidation naturalists were puzzled to account for these productions; and all kinds of solutions of the mystery were promulgated. A small hymenoptorous insect, of the Linnæan genus cynips, punctures the rising plant with its acute proboscis, and into the little cavity he

has made he insinuates an egg; in a few hours the cellular tissue swells, a tumour is produced, and the egg becomes enclosed in a fleshy chamber, which not only serves for shelter and defence, but also for food, for the larva feeds upon it. The insect is about one-fourth of an inch long, and more than half an inch in expanse from the tips of the wings. It is pale, testaceous, clothed with a very short, silky pubescence. Notwithstanding the circumstantial account given by Olivier, it has been said that the gall-nut consists of four distinct parts, having every characteristic appearance of some other kinds of nuts which are well known to be the fruits of trees; an external or cortical covering, a resinous part, a shell and a kernel; indeed, Aikin, in his "Chemical Dictionary," has described all the parts with the greatest nicety; but those who take the trouble of examining gall-nuts do not find the corresponding parts. The nuts are always found perforated, they have a complete cavity, without the slightest appearance of any kernel. The nuts present an uneven tubercular surface, they differ much from each other in size and in colour, some not being much larger than a hazel nut, others about the size of a walnut. They are different in colour, and by this are known in commerce. Two sorts are principally recognised, the one is the produce of the first collection; they are of a blackish-blue colour, hard to break, and of a heavier texture. The others are of a whiter aspect, of a pale brown or whitish colour, smooth, round, easily broken, less compact, and generally of larger size, and are considered of a superior quality. They require heat for their complete solution in water, and the solution contains a large quantity of tannin. Alcohol digested on powdered galls takes up seven parts in ten, and ether five parts. A tincture of galls is to be found both in the Dublin and the Edinburgh Pharmacopœia; they differ in strength, but they both contain all the astringency of the galls. An infusion may be made with two drachms of bruised galls to twelve ounces of boiling water, and this is often used as an injection, and sometimes as a gargle for relaxed sore throats. It has been used as an antidote where an overdose of ipecacuan has been taken, and it quickly renders it inert. A drachm of the powder of galls to about eight ounces of lard, to which opium is occasionally added, forms a most useful astringent in chronic piles, and very speedily removes pain, and the intolerable sensation of burning which is sometimes so much complained of.

LIGATURE
OF THE
LEFT CAROTID ARTERY BELOW THE OMOHYOID MUSCLE.

To the Editor of THE LANCET.

SIR:—If the following observations on the mode of tying the subclavian and left carotid arteries, are likely to facilitate these operations, they are at the service of your widely circulated publication. I am, Sir, your obedient servant,

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January 1, 1838.

First, then, with respect to the operation on the subclavian, outside the scaleni, internal to and beneath the omohyoid, and above the clavicle. Now, in this space, anatomically designated the posterior inferior triangle of the neck, the external jugular vein is liable to varieties of situation, and should be carefully avoided; in the first place, as a wound of it is embarrassing to the operator; and, secondly, ligatures on veins not being desirable.

I am aware that many suggestions have been already offered, by the different writers on the surgical anatomy of the vessels, as to the vein being drawn to the tracheal or acromial side, according to its distance from the outer border of the sterno-mastoid; yet these directions only refer to the management of the vein *after* the incision through the skin, platisma and fascia, for how could its situation be altered towards either side *whilst subjacent* to these textures and *unexposed?* But I must here be permitted to insist that *it is during* and *from the first incision* that the external jugular is principally endangered, and that the after-management of it, namely, whether it should be drawn to the tracheal or acromial side, or in what case it may, as Professor Cooper states, be indispensable to divide it after a double ligature, is *quite another*, although the next consideration. At the same time, I believe that no sufficient direction has been as yet given whereby we may safely and effectually complete our incision in depth, through skin, platisma, and fascia, and in the requisite extent, namely, from sterno-mastoid to trapezius.

If the external jugular vein lie close to the margin of sterno-mastoid, then, by commencing the incision on its acromial side, the vein may certainly be avoided; yet the incision may thus be *too limited*, it being often necessary not only to have it commencing at the outer edge, but even freely to divide the clavicular attachment of the sterno-mastoid, in order to get the ligature under the artery.

But the principal difficulty is experienced when the external jugular passes, as it frequently does, in the very centre of the space between sterno-mastoid and trapezius. Now, in this case, how is the operator usually circumstanced? He either makes an imperfect division of the parts through the *whole* length of the incision, or, *at least*, *over the vein*, or, perhaps, has recourse to two incisions, one on each side of the vein, or to repeated strokes of the knife; but, in either case, *the parts about the vein* being *imperfectly divided*, the operator next vainly essays to hook it to the tracheal or acromial side, and hence too often, I am afraid, concludes the presence of that emergency supposed to render indispensable the division and ligature of the vein.

In what manner, then, may *all* these difficulties be avoided, and in whatsoever part of the space the external jugular be situated, how may we freely divide skin, platisma, and fascia, commencing, if we please, at the mesial line, and carrying our knife uninterruptedly outwards, *ad libitum*, without the possibility of injuring the vein? I can accomplish all this by an expedient simple, and, therefore, the more valuable.

Since the external jugular vein runs down *beneath* the clavicle, to join subclavian vein, *draw down* the integuments of the neck to a sufficient distance *over* the clavicle, make your incision *at once* through skin, platisma, and fascia, *in the line of* and *upon* the clavicle, and when the pressure of the fingers, which, prior to the incision, drew down the integuments to the requisite extent over clavicle is removed, in the *retracted* condition of the same, the incision will exactly correspond with the *upper* border of the clavicle. We thus gain two points, first, by freely cutting the tegumentary structures *upon* the clavicle, we avoid the vein which runs *beneath* it; and, next, when the parts drawn down, prior to their division, are permitted to retract, we have an *effectual* wound in the *proper site*.

Secondly. Having thus proposed a new method of avoiding the external jugular vein in taking up the subclavian artery in its third stage, I proceed to consider some important points with respect to the parts concerned in the inferior operation on the *left* carotid, and especially of the anatomical relations of the thoracic duct.

Passing by its relations in the abdomen and posterior mediastinum, I shall confine myself to the parts of it connected with the operation, merely remarking, that, above the arch of the aorta, it ascends beneath the left pleura, *between* the left carotid and subclavian arteries, and along the left side of the œsophagus. Now I am induced to mention its relations here, because I apprehend that its situation in this part of its course, *external* to the carotid (as lying between it and subclavian), has led to the very serious mistake of its lying *external* to the carotid in its *entire* course as it ascends into the neck; whereas, in truth, the ascending portion of its arch gets *behind* the left carotid, and somewhat *internal* to it. If, in conjunction with this, we further consider a very important relation of the next or transverse portion of its arch, which has not been observed by the different writers on the vessels, namely, its peculiar relation to the pneumogastric nerve, we shall find that, as it passes behind this and the deep jugular vein, it is *crossed by the nerve exactly at right angles*, and that if, before we pass the aneurysm needle under the artery, we press the nerve and vein outwards, we shall, at the same time, drag the ascending portion of the arch of the thoracic duct more *outwards*, that is, *nearer to the artery* behind and *internal* to which this portion of the duct is situated; and consequently, in passing the needle, we should, by this approximation, take the very best method of endangering the ascending part of the arch, if, in truth, it can be in danger, lying, as it does, behind the sheath of the vessels.

Now I have been led to attach the more importance to these observations, because in a work, the very best on the surgical anatomy of the arteries, so far as my humble judgment and research are concerned, and one, too, of very great authority with the operating surgeon, namely, that by our esteemed professor in the *Dublin University*, Dr. Harrison, it is particularly directed "that the vein and par vagum be pressed *outwards*" before we pass the aneurysm needle. Now, it is with extreme reluctance that I am compelled to differ from so high an authority as to the management of both vein and nerve at this juncture, but I am confident that Professor Harrison, by reviewing the anatomy of the duct as I have done, namely, by inserting a blow-pipe in the vessel, and inflating it from the posterior mediastinum, after exposing its site in the neck, shall at once perceive that the ascending portion of its arch in the neck, is posterior, and somewhat *internal*, to the carotid artery; and that his caution to "remember the proximity of the thoracic duct," posteriorly and *externally*, is, in the first place inaccurate, as only the transverse and descending portion of the arch are external; and, secondly, might misguide the operator to a false security that by any effort at pressing *outwards* that which he supposes *already* and in *every part external*, he shall be further removing *every part* of the duct from danger.

Next, let the effect of pressing outwards the vagus and vein be tried, and Dr. Harrison shall find as the result, what I had the pleasure of demonstrating to several gentlemen last season, in Mr. Haydon's Anatomical Theatre, that the ascending portion of

the duct shall be thus approximated to the posterior wall of carotid in the first place, by the line of the traction from the pressure outwards of superincumbent parts, and, secondly, we should suppose, by the obstruction of the passage of its contents from the same pressure.

In what manner, then, may the aneurysm-needle be passed round the artery with perfect safety to *all* contiguous parts? I shall suppose the skin, platisma, and fascia divided; the sterno-mastoid drawn outwards; the sterno-thyroid and hyoid towards trachea; the deep cervical fascia divided; and the sheath of the vessels opened; and now I consider it a most important improvement, and an excellent adminiculum memoriæ, to divide the next proceeding, namely, the course of the needle, or eye-probe, into *two stages*, each requiring distinct management; first, its passage between vein and artery, in which we are to protect the vein; and, secondly, between the artery and pneumogastric nerve, in which we are to protect the nerve and the parts which are more directly behind the artery and its sheath, and among these the ascending and part of the transverse arch of thoracic duct. Now, in the former of these stages, I, of course, cannot object to what is absolutely necessary, namely, to so much pressure of the vein outwards as shall be sufficient to push it from the peculiar superincumbent position on the artery which exists on the left carotid; but as the needle passes farther along the side of the artery, I should advise the vein to be pressed more directly *backwards*, to avoid that portion of the transverse arch of the duct, as well as of other parts, which are now opposite the needle, and only separated by the posterior and external part of the sheath.

But it is in the next stage of the course of the needle that I would beg especially to differ from authors, and to propose a plan essentially different from what is generally directed. At this juncture I should, *in no instance*, attempt to draw the nervus vagus *outwards*, as I should, as before stated, drag that part of the duct which is behind, and *internal* to the artery, as closely as possible on the point of the needle, being merely protected by the intervening cellular sheath; but recollecting that important and beautiful relation of the nervus vagus to the transverse part of the arch of the duct, which I have *first* pointed out, viz., that it crosses the latter at right angles, I would advise that the vagus be pressed *directly backwards* against the longus colli; thus, separating the posterior wall of artery from the posterior part of sheath pressed backward by vagus, and, of course, at the same time from the parts posterior to the sheath, we can safely bring the needle between the artery and vagus, and close to the former; and now it only remains slightly to elevate the artery, as it were, *from* the sheath, and somewhat in the direction outwards on the concavity of the needle. Thus we are sure that we include nothing but the artery in the ligature, and, by the slight traction outwards, we best avoid the œsophagus, which is here internally related to the artery.

It is evident that the needle has now completed its course around the vessel with all that perfect security which the knowledge of anatomical relations can bestow.

SYMPTOMS APPARENTLY VENEREAL ARISING FROM CONSTITUTIONAL CAUSES.

To the Editor of THE LANCET.

SIR:—The following case is presented for insertion in your Journal. I am, Sir, your obedient servant,

R. L. DAVIES, M.R.C.S.
37, Dorset-street, Bryanston-square,
January 1st, 1838.

M. J., aged 20, a kept woman, and one who had been subject (to within a week of the time that I was called to attend her) to promiscuous intercourse, was affected with the following symptoms:—A yellow, uniform, and continuous discharge from the the vagina; a sore on the inner surface of the right labium; a swelling in the groin of the same side; an ulcerated sore throat, and followed in about a fortnight or three weeks, by a hard red swelling on the anterior edge of the tibia, and a similar swelling on the inner edge of the ulna.

The casual observer (and I do not mean to exclude myself from that class) would say that it was natural enough a girl placed under such circumstances should be affected with such symptoms, and without making the necessary inquiries would thoughtlessly jump to the conclusion that the case must be venereal. Let us, Sir, inquire into the causes of the symptoms in the present case, and in our investigation I think we shall see that we can more justly attribute them to an ordinary, or constitutional, than to a specific cause.

First. *Of the Discharge.*—Upon inquiry, then, I found that my patient had had repeated attacks of this symptom before; they had usually come on when her system was from any cause weak and debilitated, and had always given way to tonic medicines, astringent injections, and the recumbent posture. These discharges had always been uniform and continuous, and could only be distinguished from gonorrhœal by their colour and absence of pain in micturition. The distinction, then, here is very imperfect.

Secondly. *The Sore.*—We all know when leucorrhœal discharges occur frequently, or

are allowed to remain long, they, from being innocuous (I mean locally), become acrid and irritating, and are liable to abrade the surfaces over which they pass. Such must have been the case here, and such the origin of the ulcer, although a single one. That it was not venereal was more than probable from the circumstance of its extending more in diameter than in depth,—a superficial sore; the deep excavation with its central yellow and tenacious slough, and the ragged edges of the chancre not to be traced. I know that the appearance of venereal sores is much modified by constitutional and local peculiarities, but they have all, in whatever form they appear, the distinctive edge, and are all more or less altered and amended by the exhibition of mercury. Such was not the case here which we shall see when we come to speak of the treatment.

Thirdly. *The Bubo.*—Buboes are arranged under three heads;—the venereal, the sympathetic, and the constitutional. Acute pain, with rapidity of progress from inflammation to suppuration and ulceration, are the peculiarities of the venereal bubo. The sympathetic is generally indolent, and comparatively speaking, insensible; it usually does not suppurate, and often becomes stationary. The description of this last species was the character of the bubo in question. It was slow in its progress, only slightly painful, and did not advance beyond the stage of inflammation. That it was not constitutional, or, at least, that the constitution was not its exciting cause, was evident from the fact of its varying so much with the sore. When black wash was applied to this last-named lesion it produced the most rapid extension of it; the bubo immediately enlarged and inflamed; but, upon the removal of the lotion, and the improvement of the sore, the swelling gradually subsided. It is true leeches were applied here, but so decidedly was the swelling affected by the sore, that I feel perfectly convinced if the sore had continued to extend, the gland would, in spite of the leeches, have gone on to suppuration.

Fourthly. *The Ulcerated Throat.*—This symptom did not appear to have any connection (further than the constitution was concerned) with the others. The girl had been subject to sore throat almost from her infancy. The tonsils were chronically enlarged, and the ulcers, four or five in number, on each tonsil, were (like the sore on the labium) very superficial and indistinct. Venereal sore throat is usually well-marked and accompanied with a considerable degree of pain (here there was very little), and little or no tumefaction of the tonsils. This symptom, then, was evidently owing to a strumous habit.

Fifthly. *The Swellings on the Leg and Arm.*—I have, in more than one instance, seen this affection occur simultaneously with leucorrhœa. Women of delicate constitutions, with weak and lax fibre, are particularly liable to it. In this instance it was not known to exist until the swelling had attained a considerable size, when one night, on taking off her stockings, my patient discovered, much to her amazement and alarm, the first of the swellings in question. I believe it is not usual for erythema nodosum to occur elsewhere than upon the tibia; but here we had one on the inner edge of the ulna, and as these were on the decline, one, evidently of the same nature, but much smaller, on the calf of the leg. These swellings took the same course, and disappeared in the same manner that swellings of this nature usually do. The manner of their discovery, the circumstance of their being *seen* before *felt*, and the period at which they made their appearance, will at once disprove their connection with any venereal complaint. It would be an insult to the knowledge of the reader to attempt any further diagnosis.

Such, then, Sir, were the external symptoms of this case.

The constitutional were the following:—The circulation languid; the secretions throughout the body scanty; the tongue pale, and puckered at the sides; extreme debility, and occasional fits of syncope. No opportunity was offered me of judging of the disease from the result of connection; I had to form my opinion from the symptoms only. From the remarks which I have made upon them it will be evident what my opinion is. But, Sir, while I was satisfied in my own mind as to the nature of the case, I was not insensible of the possibility of its being venereal. The combination of such symptoms occurring in a lady of such a *chaste* habit rendered the idea of the disease being of that nature very plausible. To be on the safe side, then, I had recourse to the anti-syphilitic mode of *treatment*. After the exhibition of a purgative, six grains of blue pill, with half a grain of opium, were taken night and morning, and the sulphate of zinc injection used three times a day. This plan was continued for a week, at the end of which time the gums were slightly affected, and the discharge had nearly ceased; the sore and bubo remaining *in statu quo*. The constitutional symptoms were all aggravated. The fits of syncope occurred more frequently, and, in addition, the complexion assumed a sallow hue. I was here pressed by the gentleman in whose keeping the girl was, to say decidedly whether the disease was or was not venereal, for by my decision would he be guided in his *actions*. My reply to him was that the disease was not venereal, but I would not speak positively, and I added that if he wished a decided opinion he had better consult a more practical man. The result was I had the pleasure of meet-

ing a gentleman of the greatest experience, who, upon investigating the complaint, agreed with me that the case was not venereal, but, like me, would not give any decided opinion. This gentleman recommended pushing the mercury still further, and ordered—

Calomel, gr. j ss.;
Antimon. powder, gr. j.;
Opium, in powder, gr. ¼. Make a pill, to be taken every night. Black wash to the sore. This plan to be continued a week.

On the fourth day from the commencement of this last mode of treatment, the erythematous swellings were discovered. The sore rapidly extended, and the swelling in the groin enlarged and became red and painful. The constitutional disturbance, too, was greatly increased. This method of treatment, then, was evidently improper. I determined upon its immediate discontinuance. The calomel was omitted, the black wash rejected, and the injection (rendered stronger by the sulphate of zinc being dissolved in the decoction of oak bark) resumed. After the exhibition of a brisk purgative or two, the following form was prescribed :—

Sulph. of quinine, gr. ij.;
Dilute sulph. acid, min. xv.;
Infus. of quassia, ℥iss. Make a draught, to be taken thrice a day.

The recumbent posture was enforced, and a generous diet, with a pint of porter at dinner, and a couple of glasses of port wine after, enjoined.

The beneficial effect of this mode of treatment was very soon apparent. In about a week the discharge ceased. The sore (having lost its cause by the cessation of the discharge, and its irritability by the discontinuance of the mercury) rapidly healed The bubo dispersed soon after the disappearance of the sore. The erythematous swellings, and the ulcerated throat vanished as the constitution improved, and in about a fortnight I had the pleasure of seeing my patient convalescent.

Remarks.—This case shows the necessity of ascertaining the history of diseases. It shows, too, that while we are assisted in our diagnosis by the *group*, we should not quite forget to investigate the nature of *each abstract symptom*; and while we may be justly influenced in our opinion by the *probable*, we should not altogether lose sight of the *possible*, cause of disease. Here was a case whose symptoms were those of syphilis occurring in a girl who was constantly exposed to the cause of that disease; had we been influenced in our opinion by the apparent cause only, we should at once have pronounced the disease to have been such, and have confidently resorted to mercury. The complaint would have gained ground (as it did); we should have attributed it to our impotent mode of treatment; the mercury would have been given in larger doses, and the disease would have been further aggravated, and so on till, from being a disease of no risk, it would have been converted into one of extreme danger. Mr. Hunter, in his remarks on pseudo-syphilis, says, that it is nearly as dangerous, in some constitutions, to give mercury when the disease is not venereal, as to omit it in other cases which are really syphilitic. Many of the constitutions which put on some of the venereal symptoms when the disease is not really present, are those with which mercury seldom agrees, and commonly does harm. Mr. Hunter has seen mercury, which was exhibited for a supposed venereal ulcer of the tonsils, produce a mortification of those glands, and the patient was nearly destroyed.

Clear as this case appears to me to be, Sir, it has been looked upon with distrust by some others. If any idea of it can be formed from this imperfect sketch I shall feel obliged by receiving the opinion of the profession, my object in publishing it being more for the sake of obtaining information myself than of imparting information to others.

LABOURS OF THE

BRITISH MEDICAL ASSOCIATION.

To the Editor of THE LANCET.

SIR:—In THE LANCET for Jan. 6th, Mr. Ikin has made some remarks on the *British Medical Association*, which ought not to remain unanswered, permit me, therefore, to reply to them.

Mr. Ikin has correctly stated that it is one of the objects of this Association to suppress "one of the greatest grievances from which our body suffers, viz., the existence of prescribing chemists;" and he proceeds to complain that, notwithstanding "it is more than a twelvemonth since the *British Medical Association* was formed, we yet see no signs of better things." Now, Sir, before Mr. Ikin made this complaint, he should have remembered that if the *British Medical Association* have not been able to effect all the good it contemplates and desires, this, in a great degree, is to be attributed to the fact that a large portion of the profession, from lukewarmness and other causes, has, like himself, withheld the support and assistance which we claimed, and had a right to expect for it, and which he and the whole profession ought to have given. He and others feel their grievances, but will not put forth a helping hand towards the removal of them. Whose, then, the fault? How unbecoming this complaint.

But, although we have not yet been favoured with the support of Mr. Ikin, I here

invite him to give it, and promise that every suggestion he will make will be cordially received. It is evident that he approves of our objects; let him immediately join with us, heart and hand, and no longer be contented with a cold approval, when it is the duty of every man to make all the exertion in his power.

I am happy, Sir, to be able to inform Mr. Ikin that we are every day adding to our numbers, and receiving increased support. In several places our country friends have formed branches in community with the parent Association; and I trust Mr. Ikin will bestir himself to establish a similar one at Halifax; for it is by combination—and only by combination,—that we can obtain objects which all think so desirable, but which so few will exert themselves to obtain.

And now, Sir, to encourage Mr. Ikin to accede to my request, let me tell him what we *have* been doing since the Association was formed. It must be remembered that we are all men engaged in active practice; and from this cause unable to devote so much time as we could desire to the attainment of the objects we have in view. But we have not been altogether idle, as I shall be able to prove by a reference to the pages of your excellent periodical.

We have had committees sitting to consider the best method of remedying the abuses occurring under the altered system of Poor-Laws; they have twice sent up petitions, which, Sir, you may remember, you did us the honour to present, praying for inquiry. We have had another committee to consider the subject of quackery, and the grievances of which Mr. Ikin complains; but, from the difficulties attending these points, no further steps have yet been taken, though even these are in preparation. We have had another committee on the subject of registration, which has occupied a considerable share of attention. We have been considering the proposed Abolition of Imprisonment for Debt Bill, and the effects which it may be expected to have upon the medical profession: a petition was last session presented on this subject, and a committee is now sitting to watch the progress of the bill.

We have established a Benevolent Fund, which is progressively increasing; and in a few years will enable the Council to assist their deserving, but distressed professional brethren who may have been unable to avert calamity; and we are proposing to establish, on the principle of mutual assurance, a Pension Fund to provide for medical men during sickness; and an Annuity Fund for the benefit of themselves, their wives, children, or friends.

Lastly, Mr. Ikin will find, in the full report you so kindly gave (in No. 736) of the proceedings at our first anniversary meeting, that we have, Sir, through you, presented a petition praying for, and that we shall strenuously endeavour to obtain, such an alteration of the state of the profession, as will give to its members the power of electing a Senatus, or Governing Body, which alone can preserve to them their rights, while it shall protect the public from the gross abuses which Mr. Ikin so justly deplores the existence of.

These, Mr. Editor, form some, though not all, of the results of our labours; and I shall be highly gratified if they prove sufficient to induce Mr. Ikin, and many others, to give us that assistance which he and they could so competently contribute. I am, Sir, your obliged and obedient servant,

WM. EALES.

Southwark, Jan. 10, 1838.

A CLERGYMAN PRACTISING MEDICINE.

To the Editor of THE LANCET.

SIR:—I have read, with much pleasure, the judicious and well-timed remarks contained in the letter of Mr. Ikin, in your Journal of last week, and I sincerely hope that his call will be responded to by the members of the profession, and that some active measures will be adopted to check the growing evil of quackery in all its forms, and afford to the educated practitioners of this country protection against a system which bids fair seriously to injure their interests.

The following statement will supply an illustration of the total inefficiency of the Company of Apothecaries in preventing the illegal practices of uneducated men, and will point out the necessity of some legislative enactment to supersede the useless bill of 1815.

The Rev. Mr. Staggall, the curate of Ashfield, in this county, and diocese of Ely, has been practising for the last five years as a medical practitioner, at Rattlesdun, near this place, and seven miles from his cure. As the charges of the Rev. Gentleman, particularly in midwifery, which forms a considerable part of his practice, are somewhat less than half of those of the regular practitioners in his neighbourhood, a representation was made of the circumstance, some time ago, to the solicitor of the Apothecaries' Company, who replied, that Mr. Staggall, claimed exemption from the Act because he had been in practice previous to 1815, and that if the medical gentlemen would bring forward evidence to disprove this, the Company would institute proceedings; if not, Mr. Upton stated it as his opinion, that the Rev. Gentleman might practise *legally*. Some inquiry having been made, it was found that, in 1807 or 1808, Mr. Staggall was an apprentice in a grocer's shop in this town, and that after-

wards he went to sea, but no proof could be obtained of his having either been educated as a medical man, or of his ever having practised as such. As negative evidence of this kind would be of no use in the case, and as the Company *considerately* threw the task of obtaining evidence upon the medical men, another plan was adopted, and a memorial signed by three medical practitioners, was presented to the Bishop of Ely, who returned an answer, stating that he could not interfere, as no complaint was made of the Rev. Gentlemen having neglected his clerical duties.

According to these decisions, it is quite clear that we have no protection afforded to us by the Apothecaries' Company, and that any man who *says* that he practised before 1815, may subsequently take orders, and unite both professions, and that he will be undisturbed in his practice unless the medical men in his neighbourhood choose to be at the trouble and expense of instituting legal proceedings themselves; I should feel obliged, Mr. Editor, if you will state your opinion of the law which affects this case.

I also wish to be informed, whether the Act of 1815 is not infringed by an impudent quack *visiting* his patients, and taking with him a large quantity of medicine, for which he receives his fee, as I could adduce instances of such having been the case in this neighbourhood.

With regard to the serious evil of chemists being allowed to prescribe for their patients, there can be no doubt of its being carried to an almost incredible extent; and I perfectly agree with Mr. Ikin, that the only remedy for the evil is a prompt and energetic movement of the regularly educated members of the profession.

Let petitions be sent in, upon the meeting of Parliament, from all parts of the kingdom, praying for a full inquiry into the amount of protection afforded to the profession by the Act of 1815, and the manner in which the provisions of that Act have been carried out. Let the world see, that, instead of providing for the *safety* of the public, the only effect of that Act has been to establish a monopoly alike degrading to the character of the profession, and ineffective in protecting its interests. I trust (if such a thing is practicable) that some member of the House of Commons will move for a return of the number of the prosecutions instituted by the Company since 1815; and also the amount of fees which have been paid since that time by the Licentiates, and the manner in which that money has been appropriated. I am, Sir, your obedient servant,

C. R. Bree, Surgeon.

Stowmarket, Jan. 8, 1838.

VERDICT OF "WILFUL MURDER"

AGAINST A PUERPERAL-MANIA PATIENT.

APPEAL TO THE PROFESSION.

To the Editor of The Lancet.

Sir:—Another and most flagrant instance of the mischief arising from non-medical coroners has occurred last week. In the "Weekly Dispatch" of last Sunday (Jan. 7th) is a report of a coroner's inquest held on the body of an infant, a month old, killed by its mother, Elizabeth Hodges, the wife of a butcher residing in Ivy-lane.

From the evidence given at the inquest the case was most clearly one of puerperal mania on the part of the mother. Yet the Coroner, in his merciful *wisdom*, in summing up to the jury, observed, "That no doubt a jury in another court would, upon the evidence, take a merciful consideration of the case, and judge the poor woman to be confined as a lunatic for life!"

The woman's case was, I repeat, most evidently one of puerperal mania; and the delusion described in evidence of "the devil continually tempting her to destroy her child," is one of the most frequent attendants of that dreadful disease. Three years ago I had a case of the same description, where the patient attempted self-destruction. She also alleged that a demon was continually appearing to her, in dreams, insisting on the destruction of herself and her infant. And many of your readers must have met with similar cases. Gooch mentions this as a prominent symptom, giving most instructive illustrations, at the same time saying that they generally recover. My patient recovered her reason completely, and is now a valuable member of society.

In the sacred name of humanity, and for the honour of our profession, I call upon its members, and more especially on the surgeon who gave evidence on the inquest (Mr. Hay of Newgate-street), by proper evidence on the poor woman's trial, to prove the usually transient nature of puerperal mania, and save this wretched woman from the frightful doom, worse than death, which hangs over her,—of incarceration for life in a madhouse.

An almost confident prognosis might be given that the patient's reason would be restored by proper treatment. Who but would shudder at a verdict of "Wilful Murder" (with imprisonment for life as a lunatic) against a person committing homicide in the delirium of brain fever! Where is the pathological distinction to warrant it in this case? Yet a verdict of "Wilful Murder" was returned, *under the especial direction of the Coroner*, who, in the plenitude of his ignorance, would add the rest.

I confidently hope that through the medium of your widely-circulated Journal, this case will attract the attention of those who

may have it in their power to save this poor woman, and avert from the medical profession of England the indelible disgrace which the anticipated verdict, in a case of puerperal mania, would stamp upon it. I remain, Sir, your obedient servant,

J. M. BROWN, M.R.C.S.

Sidmouth, Jan. 9th, 1838.

CONTROLLING POWER OF VACCINATION.

To the Editor of THE LANCET.

SIR:—Unfortunately I did not see THE LANCET of Sept. 30, till this day, but I hope you will not consider it too late for me to ask Mr. Allison, of Retford, how he can possibly expect an answer to a question of such paramount absurdity as the one he propounds, viz.,—"how I know that my patients would not have died from small-pox if they had not been previously vaccinated;" as if I had made any such attempt at divination. Mr. Allison says, 1st. I have never known a reason for concluding "that vaccination has no power of preventing, or of even modifying variola, if not had recourse to before the system has become contaminated." 2nd. "But I have known good reasons for supposing the contrary." With regard to his first declaration, I may be allowed to say, that he has neither seen the facts stated in my first letter (which he has quoted from), nor that in Mr. Bree's, of Stowmarket, or he would have seen more than one good reason for the conclusion he dissents from. And, with regard to the second, I think he would have better elucidated the subject by giving his cases, and by thus allowing others to draw the inference, than by simply stating, "I have known good reasons for supposing the contrary."

You will be pleased to observe, that the question at issue is *not* whether cow-pox will prevent or modify small-pox, about which, I presume, there are no two opinions in the profession, but "has vaccination the power of preventing or of even modifying variola, if not had recourse to before the system has become contaminated."

If Mr. Allison has any facts to offer in elucidation of this point (from which he has so irrelevantly digressed in a mail-coach reverie, on common-place observations about the progress of a vaccine pustule), let them be fairly stated, and none of your readers shall give them a more respectful consideration than myself; but I think it too much for Mr. Allison to expect that time could be taken up in replying to matter so foreign from the point as that which swells his letter on "the controlling power of vaccination." I am, Sir, your obedient servant,

THOMAS PRITCHARD.

Hereford, Oct. 30, 1837.

PLAN OF SUPPLYING MEDICAL RELIEF TO THE SICK-POOR AT GIBRALTAR.

To the Editor of THE LANCET.

SIR:—Although not belonging to the medical profession, I have, like most other people, occasionally read your ably-conducted, and widely-circulated periodical, and have been delighted to see the bold, manly, uncompromising stand you have upon all occasions made when either the honour of your medical brethren, or the interests of the sick-poor, have been the subject of discussion. I would, therefore, in order to put an end to the discreditable and degrading system of penny-club and poor law union medical competition, which you all seem to deplore and complain of, beg leave to recommend to your notice the plan adopted at Gibraltar, as worthy of imitation.

The civil population of Gibraltar, I was informed when there in the summer, amounts to between 14 and 15,000 souls, and Government has liberally appointed five assistant staff-surgeons to attend to the wants of the *Jew* and other paupers, by which means infectious diseases are detected at once, and the sick-poor are promptly and properly attended to under the unremitting superintendence of an inspector-general of hospitals and quarantine, at a salary of £850 per annum.

Sir William Pym, I notice, in his evidence before the Committee of Inquiry on Colonial Expenditure, on the 14th of March, 1834, endeavours to make it appear that this establishment is excessive; but the authorities, in my opinion, have very properly decided against him. And let me ask you, Mr. Editor, what signifies the trifling expense of £1785 13s. 6d.* per annum in comparison with the beneficial ends that are attained.

Now, cannot you, Mr. Editor, in your place, as a member of the Legislature, get some comprehensive plan of this kind adopted throughout England, in place of the present paltry contract system, by which means your profession would be benefitted, and many deserving individuals brought into notice who are now doomed to waste the best part of their days in unprofitable obscurity.

There would be no lack of candidates for such appointments I am sure, and the heads

* One inspector, at 43s. per day, forage, rations, &c., £850; house rent free.

One assistant, at 10s. a day, forage, and extra pay for attending Provost Prison, £278 11s.; house rent free.

One ditto, at 10s., rations, &c., £198 10s.; quarters in barracks.

Three at 7s. 6d., rations, &c., £458 12s. 6d.; quarters in barracks.—Total, £1785 13s. 6d.

of the medical departments of the public service, I am confident, would willingly lend their assistance to forward an object so beneficial and truly philanthropic. Your obedient servant,

VIATOR.

Gibraltar, Jan. 7, 1838.

HOSPITAL ACCIDENT CASE SOCIETY.

To the Editor of THE LANCET.

SIR:—In your number for December last, an article appeared recommending an Association of non-medical individuals to visit the inmates of hospitals, and to make inquiries after such matters as are calculated to cheer and tranquillise the mind; to communicate with their families, and relieve them if in great need.

Now, Sir, I shall feel greatly obliged if you will have the kindness to inform your correspondent, and the humane public generally, through your excellent periodical, that a Society of a nearly similar nature, does exist. It is denominated, *The Hospital Accident Case Society*, and is formed for the purpose of relieving the families of the inmates of the accident wards of all the metropolitan hospitals at their dwellings, by giving them bread and meat, coals, potatoes, &c., and which relief is continued until the man is enabled to resume his labour. To carry out the operations of the Society, an agent is sent to the accident wards, and, if any case of real distress is found, it is immediately relieved.

The Society, since its commencement in 1835, has done some good, as the enclosed documents will show; but merely from its not having been more generally known, its means have been exceedingly limited, and far beneath the many claims upon its funds, arising from accident in the metropolis, but the committee *have done what they could*. May I be permitted to add, that the committee meet at the Langbourn Reading-rooms, Ball-alley, Lombard-street, every first Tuesday evening in the month, where every information may be obtained, prospectuses supplied, and where subscriptions will be most thankfully received. The Secretary will be most happy to receive any communications, addressed to the Committee-room, which shall have his most prompt and best attention. Yours most respectfully,

RICH. CROXTON.

Langbourn Rooms, Ball-alley, Lombard-st.
Jan. 8, 1838.

STUDY OF ANATOMY IN EDINBURGH.

To the Editor of THE LANCET.

SIR:—Conceiving it fair to transmit to you, who took such an active part in forwarding the interests of anatomy, not only before and during the passing of the Anatomy Bill, but also since the Bill came into operation, an account of its success, I beg to give you a few plain facts, which require no comment, upon the alteration of affairs with regard to anatomy in this city.

For many years prior to the passing of the Bill, bodies could hardly be procured in a number sufficient to carry on the lectures upon anatomy and surgery. Dissection was then little prosecuted by the students, in the first place, on account of the scarcity of subjects; and, secondly, on account of the high price of the bodies not meeting their ability to purchase them. No sooner was the Bill passed than the situation of affairs improved, and in no small degree. But the object of the Bill was frustrated, and its easy and efficient working considered to be thwarted by the inspector, whose conduct, unfortunately, not only cramped the efforts of zealous teachers, but tended much to discourage diligent students. Frequent meetings and petitions of teachers and students ensued, for the various dissecting-rooms were often empty when bodies were abundant, and great joy prevailed when his resignation was given in. Since that happy day anatomy has advanced much, and the supply of subjects has amply met the large demand. Not only are the rooms well supplied, but teachers are enabled to give two separate lectures daily on anatomy, the one adapted to a junior, the other to a more advanced class. In addition to these, lectures are weekly delivered upon operative surgery, and on the nervous system, for which additional lectures no additional fees are paid. They are numerously and spontaneously attended, and gratefully received, forming a strong contrast to those anatomical classes where attendance is compulsory. Previous to the passing of the Anatomy Bill the number of bodies did not meet the necessary demand for the teachers, and the price of an adult subject varied from £16 to £22. After that Bill came into play, and during Dr. Craigie's inspectorship, they nearly met the students' demand, and averaged in price £5. Since his resignation, at the latter end of 1835, there has been an *ample* supply, in *all* the rooms, at the low price of £3.

Our students now have every facility, not only of prosecuting their anatomical studies, but also of practically studying operative surgery, and at a cost very little higher than the vaunted economy of foreign schools; and these advantages might be still more improved, and these opportunities still more extended, if the unfair system of monopoly

did not still exist in our University. Every unclaimed body which dies in the Infirmary and is unopened, is sent, not to the teachers in turn, or in a ratio corresponding with the number of their students, but to the anatomical rooms of the University, sent there indiscriminately, unwished for, and with the knowledge that they will be wasted. Those rooms are filled with bodies, from many of which no one ever derives any benefit. They cannot all be used. But monopoly is there the maxim, and, like the dog in the manger, others are prevented from getting what is useless to themselves. The next month, however, will, in all probability, if the Town Council do their duty, break up the unfair system of self-election to the management of the Infirmary. The unclaimed bodies which are made the subjects of inspection in the Infirmary are, indeed, distributed to the extra-collegiate lecturers, but those bodies are almost always in such a mangled state as to be nearly useless. The College Professor refuses to admit an opened body. Still, however, the voluntarily-attended extra-collegiate classes are flourishing, and, through the exertions of the private lecturers, Edinburgh bids fair to resume her ancient pre-eminence. I am, Sir, your obedient servant,

EDINENSIS.

Edinburgh, Dec. 24th, 1838.

ON THE

AUSCULTATION OF THE HEART.

BY DR. HOPE.

RELATIVE SITUATION OF THE DIFFERENT VISCERA WITHIN THE THORAX.

THE heart is moveable; its most fixed point is about the situation occupied by the pulmonary artery. When a patient lies on his back, this point corresponds to the interspace between the second and third ribs, a little to the left of the sternum. When he lies over to either side, this point may pass a little over to that side. If he is in the upright position, it gravitates down to the level of the third rib; a line drawn opposite to the lower margin of the third rib passes, as near as possible, across the pulmonic valves. The aortic valves are behind these, but are from about half an inch to one inch lower down. In the interspace between the second and third ribs, the sound of the pulmonic valves is more distinctly heard than elsewhere, for at this point the vessel is nearest the surface, and the contained column of fluid within it transmits the sound; the second sound of the heart is also heard most loudly about this point.

The aorta ascends from behind, curves forward anteriorly to form its arch, and approaches near the surface, a little to the right of the pulmonary artery, and at this point the sound of its valves is best heard. Two thirds of the heart lie to the left of the sternum; its apex beats between the fifth and sixth ribs. This, as a general rule, may be correct, to which, however, occasional exceptions arise, as from one inch to one inch and a half to the inside of the mammary nipple.

The relative position which the lungs occupy to the heart is important. At the superior portion of the thorax, there is an interspace of about two inches between the inner border of each lung; but as they descend they recede farther outwardly from each other, and leave an interspace occupied by the apex and a portion of the right ventricle of the heart. In listening for the sounds of the heart, the ear should be applied over that portion of the parietes of the chest with which the heart is nearest in contact, and where it is not covered by the lungs, as their contents are bad conductors of sounds. The right auricle is at the summit of the base of the heart, but situated anteriorly. The greater portion of the left auricle lies posteriorly, its appendix being almost the only part seen; the mitral valve is somewhat lower than the auricular appendix. The tricuspid valve occupies a similar situation on the right side. The sounds produced in these valves are more distinctly heard below this point, where the heart is less overlapped by the lungs, for the reasons given above. When the heart is enlarged it changes its position, and becomes rounder in shape; its axis is considerably more oblique, and, as the diaphragm is less yielding, it presses upon the lungs, and there is larger extent of its surface in contact with the parietes of the chest. Percussion will detect its extent. Notwithstanding the opposing power of the diaphragm, the heart, when in this state, descends somewhat, and its apex may be not uncommonly felt beating between the sixth and seventh ribs. If hydropericardium exists to a considerable extent, you may also expect that there will be a considerable surface of dullness on percussion. The sac of the pericardium in these cases is generally cylindrical in its shape, and the effused fluid is generally contained in its anterior portion.

Action of the Heart.—Two sounds accompany the action of the heart.

The first is dull, and somewhat prolonged in permanence; it accompanies the contraction of the ventricle, and is synchorous with the pulse.

The second is short and clicking.

Assuming a previous state of repose, the first motion of the heart is the contraction of the auricles, immediately after which a short smart movement takes place, and almost continuous with this follows the con-

traction of the ventricles. This is followed by the diastole; a moment of complete repose ensues, towards the termination of which the contractile action of the auricle recommences. The contraction of the ventricles is synchronous with the pulse along the line of arterial pulsation near the heart; but is (if the expression might be allowed) perhaps a thought later in the remoter regions of the circulation. The ventricular contraction, the pulse, and the impulse, are synchronous with each other. During the diastole it is that the second sound of the heart is produced. Following the theory laid down by Laennec, and supposing a second of time to be occupied in the production of each complete action of the heart, the ventricular contraction would occupy half that second, the ventricular diastole one-fourth, and the period of repose one-fourth; at the termination perhaps of one-half of the period of repose the auricular contraction recommences. Laennec gives to the diastole a shade less of time, and to the period of repose a shade more, but the above divisions will serve all useful purposes. The distended auricles are stimulated by their state of distention to contract, this propels a certain quantity of blood into the ventricles, which were previously in a state of fullness, but not of distention. The muscular action of the auricles is here required to distend them. Being simultaneously stimulated, they contract, and empty themselves.

Respecting the causes of impulse, it may be stated, that if a dog's heart be removed quickly from the thorax, and laid upon a table, it will present a flattened appearance. If it be pinched, it becomes more rounded in shape, and the apex is tilted upwards; this latter symptom occurs from the base becoming heavier, and, therefore, more fixed; and this is similar to what takes place in the natural state; the auricles then form a pretty solid fulcrum, the apex becomes rounder, and is tilted forwards. With regard to the causes of diastole, it may be said, that the ventricles are not filled by muscular contraction. The diastole takes place from relaxation of the muscle, favoured by influx of blood from the auricles, which are now fuller than at other times, and by the width of the auriculo-ventricular openings.

The muscular system in general requires repose, and the heart enjoys a repose of nearly twelve hours out of the twenty-four; the diastole of the heart is to be regarded as relaxation in other muscles is, as a state of repose. Respecting the sounds of the heart, Dr. Hope considered that the true demonstration on which the first sound rested, amounted almost to a certainty; he (Dr. H.) considers that it is caused by the extension of the walls of the heart (not the *bruit musculaire*), added to by the shutting of the auricular valves, from the stretching of the chordæ tendineæ, and probably continued by the *bruit musculaire*, or motions of the fluids. It is difficult, added Dr. Hope, to insist on these points, owing to the difficulties which exist in separating these causes, but some pathological facts fully bear out these opinions. The second sound Dr. Hope considers as demonstrated to depend on the closure of the sigmoid valves.

PATHOLOGICAL PHENOMENA OF THE HEART.

In *Hypertrophy* of this organ, the walls are thickened, but the cavity is of the natural size. The *impulse* of action is strong and slow, from the density of the parietes, and there is an accompanying heaving motion from the power of the systole, whilst the sounds are diminished in volume. The *first sound* is dull, from the slowness of muscular extension; the *second sound*, also, is dull, from the slowness of diastolic action, and reaction on the sigmoidal valves becomes equally clear.

In *Simple Dilatation*, the cavity of the heart is enlarged, and the walls are not attenuated. The *impulse* is smart and sudden, and there is no heaving. The muscular action is less vigorous, but the action is smarter. In this condition of the heart, the *first sound* is louder, from the muscle being attenuated in structure, but not deficient in power. In cases where the thinness and weakness of the parietes is very great, the sound is diminished. The *second sound* is louder, the diastolic action being very smart, and the reaction on the sigmoidal valves becomes so too.

Hypertrophy with Dilatation.—Here the cavities of the heart are enlarged, whilst the walls are increased in size and strength, this affection being a compound of the two former ones. The *impulse* is strong, heaving, and sudden. The *first sound* is an increased one, from the considerable increase of muscular extension. Dr. Hope believes, that in cases of this kind he has heard the sound louder than in any other affection of the heart. The *second sound* is louder, from the diastole being sudden and and forcible, and the reaction on the sigmoidal valves is sudden also. Between these there are, of course, intermediate stages.

Valvular Murmurs.—Laennec discovered that when an obstruction existed in the sigmoid valves, the systole of the ventricle produced a sound; and that when such was present in the auriculo-ventricular valve, a sound was produced during the diastole. In 1825 Dr. Hope discovered that when the auriculo-ventricular valve was patescent, another sound was produced during the systole of the ventricle, and soon afterwards he found, that if the sigmoid valves did not shut close, a similar sound was produced during the diastole. From

these facts it may be stated, that there may be a double murmur connected with each valve. This has been proved correct by instituted experiments.

Cause of Murmurs.—If you throw a large stone into a small stream, a noise and ripple are produced; if fluid is transmitted through a rough tube a sound is produced; the stream is broken, and the particles of fluid are thrown into vibrations which become sonorous. The same results obtain in the heart. A rugged obstruction produces a rougher and a louder sound; increased velocity of the fluid increases the intensity of the murmur. A murmur of regurgitation may be produced in the auriculo-ventricular valve when no murmur is produced as blood flows naturally through; when there is a chink with only an aperture of the size of a crow-quill, powerful ventricular contraction will produce it. Frequently no disease exist but a shortening of the chordæ tendineæ which causes a murmur when, during the systole of the ventricle the valve is held open, from the chords being too short to admit of its closure; occasionally you will find a sound produced from the auriculo-ventricular opening being dilated, and the valve being, from this cause, unable to close it.

Situations in which these Sounds are to be studied, so as to distinguish them from one another.—If, on applying the stethoscope to the interspace between the second and third ribs, close to the sternum, a loud superficial hissing murmur is heard, it is almost certainly produced in the pulmonic valves, as in that situation the artery is near the surface, and this lisping or hissing sound is produced by a superficial obstruction; and if this sound be heard more feebly over the aorta, this opinion is strengthened and corroborated. If you listen over the aorta, and hear distinctly a murmur, whilst there is none over the pulmonary artery, you may conclude that this murmur arises from the aortic valves. If the stethoscope be applied opposite to the valves, the sounds cannot be so readily distinguished as above them, where the vessels approach nearer to the surface. In situations so high as these you can barely hear a sound produced in the auriculo-ventricular valve. In order to hear the murmurs in the mitral valves, you must listen below the situation they occupy. At the point where the ventricles come in contact with the walls of the chest, the murmurs produced in the sigmoid valves can hardly be heard, and from this circumstance, the stethoscopist must listen above one set of valves and below the other. If there is existent disease in both valves, it may be distinguished by a murmur being heard in both situations; the nature of the murmur may also be different in the two valves from each other.

Qualities of Murmurs.—The various sounds detected in these murmurs have received different appellatives, such as bellows sounds, rasping sounds, filing sounds, and sawing sounds. Laennec and others have imagined that these sounds depended on the differences in the nature of the obstruction. Dr. Hope has found that these distinctive sounds are partly imaginary, but are mainly dependent on the distance of the ear from the surface on which they are produced. If valvular disease exists in the pulmonary artery, the ear applied over that vessel would detect a hissing sound; over the middle of the ventricle a sawing sound would be heard; whilst at the apex of the heart the murmur would partake more of the character of a bellows sound. The sounds in the aortic valves are not so hissing as are those in the pulmonic valves; their murmurs give a scouring sound, from their being situated more deeply beneath the surface. In the mitral valve, which is lower still, there is more of the bellows sound; the murmur produced by regurgitation into the ventricle, is generally a soft and prolonged sighing sound. The stethoscopic sounds are also, to a certain extent, modified by the nature of the obstruction; a very rough cause, for instance, producing a rasping sound. If the obstruction be considerable, you will not only meet with murmuring sounds, but you will also have an accompanying thrill or tremor; this is also sometimes perceptible in the arteries, where there there is valvular aortic disease; or where the inner coat of the artery is roughened, and the blood is propelled violently through it from powerful contraction of the heart.

In some cases of hyperthrophy with dilatation, a murmur is produced during the contraction of the ventricle; the heart becomes rounded, and the currents of blood in the ventricles are conveyed at more obtuse angles, so as to run together at the aperture, and thus a murmur is caused. Under these circumstances the pulse will be weak, whilst the action of the ventricle is very strong, its power being exerted on itself.

But you may have murmurings and thrillings in the heart and arteries, independent of any existing disease, and this is a very common symptom in patients who are labouring under nervous anæmia; these will be found to correspond in pulsation with the systole of the ventricle, and they are referable to the moving power of the blood in the vessels. In some experiments made upon dogs during the period of reaction from loss of blood, when the pulse had risen from 120 to 190, this murmur and thrill in the arteries were perceived. If, after it had disappeared, the dog was held with his head downwards, which made him faint, the murmur returned. In these animals the blood was found to be in a much thin-

ner state than natural, the quantity of fibrine having been very much decreased, and that of the watery portion very much increased, which caused the most murmur. In cases of anemia, with nervous palpitation, this murmur is a very common symptom, being easily produced in a moment by any mental emotion, or slight physical exertion. How are these murmers to be distinguished from those caused by organic disease? Why, first, by their occurrence in those who are labouring under this species of disease; and, secondly, by their want of permanency, and by their subsiding when the patient becomes calm and tranquil.

Musical sounds are not unfrequently heard, principally opposite to the aortic orifice, and occasionally, but very rarely, in the arteries. They are generally produced by some accidental valvular configuration.

POISONING BY ARSENIC.

EXHUMATION OF THE BODY THREE YEARS AFTER INTERMENT,

AND DISCOVERY OF THE ARSENICAL POISON BY MEANS OF CHEMICAL ANALYSIS.

Translated from the Report of M. M. Barruel, Henry, and Ollivier of Angers, as published in the "Annales d'Hygiene Publ. et de Medecine Legale," Nov. 1837.

WITH OBSERVATIONS.

By Thomas Henry Burgess, *M.D., &c.*

Not many years ago (says the reporter) a generally received opinion gave a deceitful security to the criminal who chose poison for the destruction of his victim. It was then believed that a very short time after interment sufficed, in order that the decomposition of the body might render all evidence of crime abortive. It was believed that the medico-legal researches at that time were inconclusive, on account of the impossibility of again recognising the body upon which the crime was committed.

But the experiments of M. Orfila, with regard to this important question ("Archives Gen. de Med.," tom. xvii., "Traité des exhumations judiciares," t. ii., p. 265), have demonstrated that the limits of science are not all so circumscribed as was vulgarly believed; they have discovered that an enlightened investigation can, on the contrary, detect the poison, even when many years have rolled by, and when the progress of the decomposition of the body had almost effaced the appearance of those very organs which had received the poisonous substance. In fine, many cases of poisoning in which this substance had been found after the lapse of one month, three months, nine months, and even seven years interment, have appeared to confirm the correctness of the signal results of M. Orfila. They inculcate the great utility that there is in the publicity of similar examples. They cannot too frequently point out, that a crime so hidden as that of poisoning is, cannot henceforward escape the scrutinising eye of science; and it is with the view of giving a new example of the discovery of arsenic in the human body, after a *long lapse of time*, that I come forward to-day to relate the following fact:—

In the month of November, 1833, the widow Lamothe, residing at Saint Martin sur Oreuse, in the department of the Yonne, confided to some individuals there, that the widow Chevalier had appointed her her general legatee, adding, that she hoped very soon to enjoy her fortune; "because," said she, "the widow Chevalier *cannot live very long*." In a word, a few days had scarcely passed by, when the latter died rather suddenly, after having given evidence of severe and repeated vomitings, and aggravated colicky pains in the region of the stomach. Public rumour immediately accused the widow Lamothe of having poisoned the widow Chevalier; but these serious reports did not come to the ears of justice. However, from that period, all the inhabitants of the village remained convinced of the culpability of the woman Lamothe, and the suspicions of her crime were not at all forgotten, although three years had already elapsed, when a broil broke out in the house of the woman C——, one of the neighbours of the widow Lamothe. This event immediately revived the suspicions of the past; all the circumstances were combined together; besides, in order to accuse the widow Lamothe of this new crime, a complaint was brought against her by the woman C——, who farther declared, that she was acquainted with the poisoning of the widow Chevalier, and that, if she had kept silent up to this time, it was merely from the fear with which the widow Lamothe inspired her, and whose vengeance she dreaded. Judicial investigations were immediately entered upon, and they brought to light the certainty that the widow Lamothe had possessed at the time of the widow Chevalier's death, an ounce of arsenic, the employment of which she could not by any means account for; and the physician who attended the latter in her dying moments, proved some facts which confirmed the whole. The exhumation of the body was then immediately made (Nov. 21, 1836.)

The public cemetery occupies a soil somewhat elevated, gravelly, and naturally very dry. All the proofs as to the identity of the body were perfect; it was found in a state of remarkable preservation, and was immediately conveyed to Paris, where it was submitted to our examination and to our researches, as we shall presently see.

The case being opened, we found, in the midst of grass, or straw, and shavings of wood, a packet, enveloped in white linen, without any mark, and in which was found a portion of the dead body, consisting of a headless trunk, and from which were detached the superior and inferior extremities. The general colour of the parts submitted to our examination was of a very deep sooty tint. A slight brownish earth, and some remains of a winding-sheet, adhered in many points to the surface of the trunk of the body, which was completely mummified, dry, almost inodorous, and the weight of which scarcely exceeded *two pounds*.

No crevice, no accidental opening existed upon the surface of the abdomen, the walls of which were untouched, sunk in upon the vertebral column, and covered in some manner by a large piece of thick and brownish linen, adherent to the dried integuments, and which proved, without doubt, to be part of the chemise and winding-sheet that covered the body at the time of burial. Some hair, of a reddish brown colour, still covered the pubis, and adhered intimately to the dry and withered skin of this part of the trunk. Beneath the pubis we observed a vertical cleft elongated, the borders of which were thin, angular, and blackish, without *any solution of continuity*. This opening, somewhat large and deep, was evidently the traces of the vulva, and demonstrated that the body was that of a female. No remains of the winding-sheet were observed covering the parietes of the thorax; all the ribs remained untouched; but the second, third, fourth, fifth, and sixth of each side were separated from their cartilages, which were slightly depressed with the sternum, to which they adhered. This disjunction evidently resulted from the withering of the thoracic parietes, and the retraction of which had strained the tissue of the cartilages, the latter being not all fixed to the ribs, were detached in time, in consequence of their remaining adherent to the sternum. This latter bone, the connection of which with the two first ribs, and both clavicles, did not undergo any change whatever, and upon which the abdominal integuments were continued without intermission, bounded the thoracic cavity in front.

We raised the entire of the abdominal parietes, and we appeared thus to have discovered a space to all appearance void; for the sinking down and progressive withering of all the organs, without exception, had reduced them to membranous flakes, as it were, variously commingled between each other, lying upon the vertebral column, the parietes of the hypochondriac regions, the iliac fossæ, and filling the hollow of the pelvis. In the interstices of these flakes, or layers, we observed a granulated substance, which appeared pulverulent and brownish. Thus, the liver, the spleen, the kidneys, the uterus, and its appendages, were found confounded with the remains of the stomach and intestines, in these membranous folds that we detached from the vertebræ and bones of the pelvis. We only observed upon the two first lumbar vertebræ, and to the right, consequently in the region occupied by the liver, a substance adherent to these bones, appearing waxy when cut into, and of a deep brown colour; this last trace of the liver, circumscribed to about two inches in diameter, proved that, in decaying, this organ had undergone a progressive and considerable diminution in its volume. After these details, it appears, undoubtedly, useless to add that it was impossible to discover the slightest appearance of the normal form of the various organs above-mentioned. To the right, a membranous flake, the remains of the diaphragm, imperfectly inclosed the thoracic cavity; to the left there only remained some dry and irregular shreds of the other half of the same muscle; we could not perceive any traces of the lungs, and in their place was discovered a brownish friable substance, reduced to irregular masses; the heart was represented by a blackish substance, hard, brittle, and lying upon the vertebral column, to which it adhered. Thus, as we have already stated, this membranous and leafy substance, which adhered to the internal parietes of the abdomen, was entirely removed, and with the greatest care; the vertebral column and the bones of the pelvis having been thus, in a manner, denuded, we were then enabled to prove the rounded and perfectly *non-angular* form of the pubal arch, the very considerable separation between the tuberosities of either ischium, the remarkable extent of the different diameters of the superior straight, and the excavation of the inferior cavity of the pelvis, also the very remarkable distance between the ossa ilii, characters confirmatory of the opinion already advanced, and giving proof that *the corpse, whose trunk we were about to examine, was clearly that of a woman*. All the bones were exceedingly light, and very fragile. In order to conform to the rules of the judicial ordinance, it was necessary for us to perform several chemical experiments; for, according to the decrees of this ordinance, we ought to investigate whether the detached portions of the woman Chevalier at all contained *any preparation of arsenic*, or of any other nature, to which her death might be attributed. We might in consequence presuppose the presence of many metallic substances, such as *lead, copper, antimony, tin, &c.*; and, above all, of arsenic, the existence of which was particularly suspected.

We might also, though with little semblance of truth, seek some traces of *poisonous vegetable alkalies*. In the chemical operations about to be related, we examined successively,—First, the granulated pulve-

rised substance, extracted from the abdominal cavity of the dead body; secondly, the membranous and flaky substance resulting from the desiccation of the stomach, of the intestines, and other abdominal organs. The stomach alone presented the appearance of poison. We shall pass over in silence the long and forcible details of the chemical analysis, in order to arrive at the final results which determined the presence of arsenic in this organ. * * * * The liquid product that we then obtained was of a deep brown colour; after having evaporated it very carefully, and almost to dryness, it furnished to us a blackish brown residue, friable, shining on charcoal, with an odour at first of animal empyreuma, which was immediately followed by a peculiar alliaceous odour, and like to that produced by burning arsenic. A small quantity of this residue was heated strongly with nitric and hydrochloric acid, until all the brownish substance had disappeared; and the product of this treatment being neutralised, gave a brick-red colour with the nitrate of silver, after the manner of arsenic acid, or of the arseniates. There were already some presumptions which would authorise us to suspect, in the above-named residue, the existence of an *arsenical product*. And, in order to demonstrate its presence, we separated into two parts the suspected product, and it was then submitted to the following process:—

Experiments made in order to Isolate the Arsenic.

Essay A.—The first portion, being mingled with pure potash, in alcohol, was heated by the flame of a spirit lamp until the mixture, sensibly torrefied, became completely dry and pulverulent. It was introduced in this state, with charcoal in minute division, into a small tube, closed at one extremity, and drawn out at its other extremity by the flame of the lamp; it was then submitted to the heat of the flame. When the animal matter was destroyed, we immediately perceived some metallic zones, of a steel-grey colour, which spread on the elongated part of the tube, disengaging an alliaceous odour, and forming there a brilliant and mirror-like ring;—*this was arsenic in the metallic state.*

Essay B.—The second portion of the residue, triturated with a small portion of pure potash, and four or five times its weight of the *nitrate* of the same base, was gradually heated, afterwards calcined. We extracted from the cooled tube a whitish saline mass, which distilled water was enabled to dissolve completely. The solution was treated with the nitrate of silver, very pure, and it was immediately observed to yield an abundant brick-red precipitate, which, being collected, washed, and dried, appeared from its weight, and the composition of the salt of silver, to contain from six to eight grains of arsenic. This precipitate, soluble in ammonia, in nitric acid, and yielding with charcoal a strong alliaceous odour, *was the arseniate of silver;* it now became important to isolate equally the arsenic in the metallic state. We then transformed the greater part of this salt into the arseniate of potash, mingling the whole with powdered charcoal, drying it, and exposing to the flame of a spirit-lamp, in a small tube, drawn out like the preceding. Some zones, of a steel-grey colour, were not long in appearing on the elongated portion of the tube, and forming there a metallic ring, all the characters of which were those of *arsenic*. This metal was joined to that obtained by experiment A, already described. In fine, in order that no doubt should remain regarding the nature of this metal, we placed a very small quantity of it in a tube, which was gradually heated, leaving access to the external air; the metal immediately lost its colour and its lustre completely; it disengaged an alliaceous odour, and became volatilised on forming a thin white crust, the crystallisation of which was distinct.* The tube which contained it being cut with a file, was boiled in distilled water. The liquid, which was previously very limpid, assumed at the end of a few minutes, by the addition of a little hydrosulphuric acid, a yellowish tint; and, by the addition of the muriatic acid, it readily produced a yellowish flocculent precipitate of the sulphuret of arsenic, soluble in ammonia. We laid before the Tribunal with the report, as specimens of conviction,—

1st. A tube, containing arsenic, reduced to the metallic state.

2nd. A tube containing the arseniate of silver.

3rd. Another, with the precipitate of the sulphuret of arsenic.

Conclusions.—After the experiments above-mentioned, we concluded that the organic remains taken from the abdomen of the woman Chevalier, did not contain any trace of *vegetable* poison, or of mineral poison having for their base *lead, copper, tin,* or *antimony*. But we affirm that they contain a very evident quantity of an arsenical product, as to the existence of which it would

* Dr. Christison recommends the metallic crust to be chased up and down the tube by the aid of a spirit-lamp flame, until it is all oxidated, when little octahedral crystals of adamantine lustre are formed, upon which, either with the naked eye, or with the aid of a common lens, triangular facettes may be distinguished. Dr. C. also adds, that the metal, when sublimed in a tube, may be recognised by its appearance, even when it does not exceed the *two hundred and eightieth* part of a grain. Mr. Kemp, of Edinburgh, is of the same opinion.

be impossible to raise the slightest doubt, and that we discovered it in a sufficiently considerable proportion, in order that there could not be a doubt but that the death of the woman Chevalier had been the result of *poisoning by arsenic.*†

A case of this kind occurred not long since in one of the provincial towns. A young man, aged 24, was married for several years; his wife, being a woman of violent temper, prevented him from living with her during the last two years; he, during this period, held a situation in a gentleman's family, and was frequently visited by his wife, who never failed to abuse him in every manner possible. Being irritated and enraged by this treatment on the part of his wife, he resolved on effecting her death, it being, in his estimation, the only possible means of freeing himself from such constant and unmerited abuse. To put his design into execution, he feigned a wish to become reconciled to her, and, in order to form plans as to their future way of livelihood, he invited her to meet him on a certain day, at a public-house, in the neighbouring town; they accordingly met, and the man called for some *warm* beer, which was brought by a girl of fourteen years of age, as far as the door of the room, when the man, who was sitting there with his wife, came forward, and, according to the girl's evidence, poured a *white powder*, which he held in his hand, into the beer, as he took it out of her hands. The woman drank this beer as given to her by her husband, and scarce ten minutes had elapsed, when she became distressed with violent vomiting, and her abdomen shortly after became *greatly swollen*; she immediately exclaimed, "I am poisoned! I am poisoned!" The husband then attempted to make his escape, but was soon arrested by the police. The stomach-pump was applied, and the stomach was relieved of its contents. The woman recovered in the course of a week, or ten days, and then attended at the prosecution of her husband, who, having pleaded guilty, was condemned and executed the third day after trial. This unfortunate man declared from the gallows that he never would have attempted to poison his wife by arsenic, were it not from the firm conviction that *he never could be discovered*, as he was always given to understand that it was *almost impossible to discover arsenic after being mixed with warm fluids*, and then drunk. The woman was evidently *saved by being overdosed*, having vomited up the arsenic. According to the girl's evidence there was about an ounce in the paper, which was poured into the beer, although the apothecary declared he only sold him, on the day before his attempt at poisoning his wife, two drachms of arsenic, for the purpose of poisoning rats, as he was told. The apothecary's apprentice was also arrested, but was let out on bail, after a few hours confinement. Here was another case bearing some analogy to that related by M. Ollivier, more particularly as regards the ideas of secrecy, of concealment, and of darkness, which the vulgar are still possessed with, regarding this drug, when used as a poison.

Dr. Christison, of Edinburgh, who, according to M. Orfila, ranks foremost in this country as a toxicologist, makes the following observations on the proper method of testing arsenic:—

The proper material (says Professor C.) for reducing the *oxide* of arsenic, is freshly ignited charcoal; with this substance the whole metal of the oxide of arsenic is disengaged. The black flux, which is usually recommended, is ineligible, if the quantity of the oxide be very small, for only a part of the metal is disengaged, the remainder being contained in the flux, probably in the form of arsenuret of potassium. I have found that two grains of the oxide reduced with the black flux at a full red heat, yield 0.635 grains of metallic crust, instead of 1.407, or somewhat less than a half of the metal they contain. I think *reduction* to be the best test, and is not liable to those fallacies supposed by Paris, Smith, Beck, &c., &c. The garlic odour is evolved by the sublimation of metallic arsenic only, never by the oxide, unless it be at the same time reduced. This test *should be discarded*; it does not *always detect arsenic when present*; and the alliaceous odour is not an *infallible proof* that it is present. Zinc powder projected on burning charcoal, gives out a similar odour. Phosphorus, phosphoric acid, and the phosphates, the same odour, according to Hanneman, and I have frequently remarked one very like it from *burning paper*. Dr. Christison is also of opinion that in organic fluids the liquid reagents must be discarded entirely unless for *trial tests*.

† By the decree of the Court of Assize of Yonne, given on the 16th of March, 1837, the widow Lamothe had been declared guilty of producing the widow Chevalier's death by poison; the jury of this tribunal having recommended her to mercy, she was condemned to hard labour for life.

MEDICAL APPOINTMENTS IN THE IRISH INFIRMARIES.

To the Editor of THE LANCET.

SIR:—As you will be doubtlessly called on, in your legislative capacity, during the present session of Parliament, to discuss and pronounce an opinion upon two very important questions, which deeply affect the interests of the great majority of the members of the medical profession in Ireland, I trust you will not deem it intrusive on my part, in thus publicly addressing you

upon subjects with which peculiar circumstances have made me conversant. The questions to which I allude are,—

First. How did it happen that the members and licentiates of the *Dublin College of Surgeons* became the monopolists of the Irish county infirmaries, and shall they be permitted to enjoy the *exclusive* right of being elected to those institutions?

Secondly. Has the conduct of the predominant party at the Irish College been of such a nature as to justify the Legislature in granting to it a *second* monopoly, to the serious injury of another branch of the profession, the apothecaries, who have *not*, I believe, been convicted of having oc:tstepped the provisions of their act of incorporation?

To the discussion of these two questions I mean to apply myself on the present occasion, and in so doing it is not my intention to introduce irrelevant matter, but to adhere closely to such facts as I can establish by reference to documents, or elucidate by the logic of common sense.

The monopoly, by the *Dublin College of Surgeons*, of the Irish county infirmaries, is not a matter of recent occurrence; on the contrary, it has been in active operation almost from the time the College itself was first established.

By an Act of the Irish Parliament, passed in the fifth and sixth years of George the Third (1765), it is provided, "That no person shall be capable of being elected a surgeon to a county infirmary in Ireland, who shall not have served a regular apprenticeship of five years to a surgeon, and have likewise undergone an examination before the surgeon-general, the visiting surgeon, the two assistants, and the resident surgeon of *Dr. Steeven's Hospital*, and the five senior surgeons of *Mercer's Hospital*, and shall produce a certificate from the said examiners, that he is in all respects qualified to be a surgeon to the said infirmary."

It is plain that this was an Act which, at the time of its adoption, must have met the approbation and support of every friend to suffering humanity, when it is recollected that previous to that period there was no tribunal by which the competency of the pretenders to surgical knowledge could be tested, and, consequently, no rule of humanity by which the health and the lives of his Majesty's subjects could be secured.

In the year 84, the *Royal College of Surgeons* was first incorporated by Royal Charter; and its members, by virtue of that intuitive or instinctive faculty, of taking care of their own interests, which appears to be a gift peculiarly enjoyed, and universally acted on by corporators, succeeded, after twelve years' experience, in the art of intrigue, by accomplishing what might perhaps be designated their first, or maiden job, with the government of the day, by getting the Examining Board of 1765 dissolved, and its functions transferred to their chartered institution. Accordingly, by the 36th of George the Third (1796), it is enacted, "That no person shall be capable of being elected surgeon to a county infirmary, who shall not have previously obtained letters testimonial of his qualification, under the seal of the *Royal College of Surgeons* in Ireland," and that *no other qualification, or examination, shall be* necessary to make any person capable of being elected surgeon to such infirmary." By an Act of the Imperial Parliament, the 54th of George the Third (1814), it is provided, "That the surgeon of the *county infirmary* shall, before he receives his salary, lay before the Grand Jury his letters testimonial from the *Royal College of Surgeons* in Ireland."

In 1818, this question was again brought before the Imperial Parliament, the *London College of Surgeons* having framed a Bill "to regulate the practice of surgery throughout the *United* Kingdom," in which a clause was introduced to set aside the monopoly previously enjoyed by the Irish College, as regarded the county infirmaries. However, on this occasion, the *Dublin College* again came off triumphant.

Mr. Peel, now Sir Robert, was then Secretary of State for Ireland, and having been plentifully *carwigged* by our ingenious and self-disinterested friends at this side of the water, viz., Crampton, Colles, and Co., and having an early leaning towards corporate monopoly, he, with a plausibility of which he is so accomplished a master, succeeded in persuading an English House of Commons of "the justice of continuing a system which experience had proved to work well for the sick-poor of Ireland!!!" By an attentive observance of the foregoing statement, it will be seen that the Irish infirmary question had been four times under the consideration of Parliament previous to 1819, and that in every instance its decisions were in favour of Irish surgery. Sir, under these circumstances it is not to be wondered at that the champions of the Irish College do boast of the superiority of their institution over the London and Edinburgh Colleges, and confidently point to the acts of the Legislature, in justification of their very unbecoming, if not groundless assumption, of what they modestly designate their "acknowledged superiority." I verily believe that many superficial thinkers, amongst the Dublin surgeons, have persuaded themselves that what is fairly attributable to misrepresentation, delusion, and chance, has been justly awarded to their body, as a tribute due to tried merit and intrinsic worth. Their notions on this point have been more than a little strengthened by the success of the deputation sent by the *College of Surgeons* last summer, to remonstrate with the Government against the first clause of the Medical Charities' Bill. Had

that Bill been passed through Parliament, with the clauses it contained in the first instance, it would have had the effect of repealing the different Acts mentioned above, and, consequently, of depriving the College of its chief buttress of monopoly, the only one upon which its precarious existence mainly depends.

I am credibly informed, that the members of the deputation, on their return to Dublin, at a meeting of the College convened for the express purpose of receiving their report of what passed in London, boasted, in no measured terms, of their success with the Government; that Lord Morpeth at once yielded to their remonstrance, and that Mr. Woulfe, the Irish Attorney-General, actually apologised for "the accidental introduction of the obnoxious clause," and the untoward, but to him unforeseen, consequences, which would have resulted from its adoption. He further assured the astute delegates, "that the object of the Bill was expressed by its name (charity), and that it should not interfere with the vested rights of the *Irish College of Surgeons.*" I cannot pledge myself for the verbal accuracy of this statement, but I have no doubt but it is substantially correct; and, judging from the omission of the "obnoxious clause" in the amended Bill, I am disposed to think that the deputation did really bully the Irish Government into an adoption of the views of the *Dublin College of Surgeons.*

Now, Sir, as an humble member of the medical profession, allow me to thank you for your formidable and effectual opposition to the Bill of last year; and I earnestly hope that you will be as vigilant when the promised "Medical Charities' Bill" of this year shall come before the House, as you were last session; and that you will take care that the two incompatible ingredients, charity and monopoly, shall not be promiscuously combined, and that the excellence of the former shall not be vitiated by its anomalous association with the latter. You may naturally inquire on what grounds the *Irish College of Surgeons* can, in the nineteenth century, presume to seek or expect a continuance of a monopoly, which carries on the face of it an insult to the London and Edinburgh Colleges?

Why, Sir, if this question were put to the modest, yet valorous, President, Dr. Jacob, (bless the mark), he would, in all probability, immediately reply pretty nearly in the following strain:—"The present system has worked very well, and there is no necessity to let in the half-educated English and Scotch practitioners." He would then vauntingly flourish the *present* by-laws of the *Dublin College* over his head, and exclaim,—"Look at *our* curriculum, at once the test of our acquirements, and the proof of our superiority; there can be no mistake about it; there it is, down in black and white. We require *three* years' attendance upon hospital practice, and also *three* courses of lectures on anatomy and physiology; when the London and Edinburgh Colleges adopt our course of education, then, and not till then, will we entertain the question of admitting their jibs into our county infirmaries. The Whig Government dare not interfere with our rights; and, even if they were base enough to do so, we would get *their* Bill kicked out in the House of Lords."

Now, with regard to the "black and white" conclusion of the *Dublin College of Surgeons*, it, *per se*, is no proof whatever of the superiority of the surgeons *already* in possession of the Irish infirmaries, inasmuch as not a single one of them has been educated in conformity with its provisions or enactments; and, indeed, if such a compliance were necessary for the education of a surgeon, I have no hesitation in saying that neither the *very learned* President himself, nor a member of the *august* Court of Censors, over which he presides, would be found qualified to take charge of a county infirmary in Ireland. Oh, Sir, what will your readers think, when I now tell them that this much-lauded curriculum is not, as yet, nine years old, having been the offspring of the "*new charter*," granted, by special application, to the members of the *Royal College of Surgeons*, in 1828. Moreover, candidates for "letters testimonial" were not required to comply with its conditions for *three* years after it came into existence. I now positively affirm, that at this moment there is not an infirmary surgeon in Ireland who has not passed his examination at the *Dublin College of Surgeons*, previous to the year 1830. If this be true, and I challenge contradiction, what becomes of the "incontrovertible" argument founded upon the undisputed and indisputable merits, and superiority of the mighty curriculum?

The next argument advanced by the Dublin monopolists, in support of their claim to the *exclusive* right to the Irish infirmaries, is the comparative strictness of the examinations at their College, and the acknowledged respectability of *their* corporation. I admit that some years back the Irish College was really respectable, and deservedly respected; but, alas! as an Irishman, I grieve to say so, the days of its glory were but of short duration. The new charter and the "ticket system" have been the cause of its ruin; the once respected College has been converted into an auxiliary to a school of a ——— description.

At present the most respectable surgeons of the *Dublin College*, viz., Carmichael, Crampton, Colles, &c., scarcely ever attend its meetings: they have withdrawn from them in disgust, leaving the entire management to the needy schoolmasters, and em-

bryo Professors, who are incessantly making by-laws on the subject of education. This mode of proceeding may be very meritorious, as regards themselves and families; but it is truly lamentable to see a national institution made subservient to private interests. As to the examination, this involves the question of competency and respectability of the examiners themselves; I will, therefore, dismiss it by asking, does any one out of the medical profession, with the exception of their immediate friends, know *who* they are, or *how* they perform their official duties? No, there are not, perhaps, seven surgeons in Dublin less known, either as practitioners, or men of scientific attainments, then the present Court of Censors of the *Royal College of Surgeons* in Ireland. No matter, they are the supporters of *the* school, and this appears to have, for some years past, been considered by the predominant party a sufficient test of their competency to discharge the important duties of examiners.

Such, Sir, is a brief but, I trust, a correct statement of the facts which relate to the Irish infirmary question; I will now leave to you, and your fellow legislators, to decide whether it is based upon such a foundation as will warrant you to support the continuance of the monopoly, and also to definitively settle whether the law be good, or otherwise, which practically proclaims the superiority of the members and licentiates of the *Dublin College of Surgeons* over those of the sister countries, who are openly declared incapable of prescribing, and otherwise conducting the treatment of the sick-poor of Ireland. I have the honour to remain your obedient servant,

ANTI-MONOPOLIST.

Dublin, Jan. 1, 1838.

CRITICISM BY DR. HOLLAND, ON DR. SMITH'S EXPOSITION OF HIS VIEWS OF ANIMAL HEAT.

To SOUTHWOOD SMITH, *Esq., M.D.*

SIR:—The second volume of your admirable work "On the Philosophy of Health," has just been transmitted to me by a friend, in consequence of containing strictures on what you consider to be my views on animal heat, and which you endeavour to show are neither "logical nor sound." Reasoning on the premises you lay down, your conclusions are correct; but it is somewhat extraordinary, that the premises are your own, and not mine, and the principles adduced in refutation of a theory, which was never entertained by me, are distinctly stated in a hundred places in my "Experimental Inquiry into the Laws of Life." Your talents, literary acquirements, and position in society, demand of me the most liberal courtesy, and far be it from me to attribute to you any intention, either to misinterpret my views, or to adopt them without acknowledgment. You are utterly incapable of either, and had your work been less worthy of approbation, your strictures would have been unnoticed. They cannot, however, be regarded with indifference, appearing in a work so superior to all others, on the popular and comprehensive subject of health, in method, in the development and illustration of principles, and in the graces of an easy, elegant, and correct style, I need not tell you how painful it is to see fallacies ascribed to ourselves, in company with so much truth and excellence; and my only request is, that you will do me justice in the second edition of your work, by giving a more accurate and faithful exposition of my views. This communication is not to prove that my theory of animal heat is correct. This is no part of my object, but simply,—

First. To show that you have altogether misinterpreted and misstated my views.

Secondly. To show, by reference to my work published subsequently to the "Experimental Inquiry." that the chemical theory of animal heat is stated in the most unequivocal terms, viz., *that animal heat is proportionate to the chemical changes which the blood undergoes in the lungs.*

Thirdly. To prove that the principles adduced in refutation of my views, are distinctly stated or illustrated in almost every page of the "Experimental Inquiry."

The following extract contains a part of your remarks which have a reference to myself:—

"These, and other analogous facts, abundantly establish the relation between the function of respiration and that of calorification, and lead to the general conclusion that the generation of animal heat is in the direct ratio of the quantity of air and blood which are brought into contact, and which act on each other in a given time. Yet an attempt has recently been made by an ingenious physiologist* to disturb this induction, and to show that the production of animal heat is not in the direct ratio of the quantity of oxygen inhaled, but in the inverse ratio of the quantity of blood exposed to this principle. This position is maintained on the following grounds:—Inspiration favours the flow of blood to the lungs; expiration retards it; consequently, if, from any causes the inspirations preponderate in number and proportion over the expirations, a greater quantity of blood than usual will be accumulated in the lungs. There are conditions of the system

* "Experimental Inquiry into the Laws of Organic and Animal Life." By George Calvert Holland, M.D.

in which this preponderance of the inspirations actually takes place; when the mind is under the influence of certain emotions; for example, as when it is depressed by anxiety and fear. In this state the inspirations are more frequent and more complete than the expirations; it is a state of continual sighing. In like manner, in certain diseases, such as asthma, the inspirations greatly preponderate, both in frequency and energy, over the expirations. In such conditions of the system the blood accumulates in preternatural quantity in all the internal organs, but more especially in the lungs; and two consequences follow: first, there is a remarkable diminution in the energy of all the vital actions; and, secondly, there is a proportionate diminution in the production of animal heat. On the contrary, as it is the effect of inspiration to facilitate the motion of the blood through the lungs, so it is the effect of expiration to retard it; hence, when the expirations preponderate, the opposite state of the system is induced; all the vital actions are performed with increased energy; the heart beats with unusual vigor; the pulse becomes quick and strong; a larger quantity of blood is determined to the surface of the body, and this excited state of the system is always attended with an augmentation of the temperature. As in the first state there is a greater, and in the second a smaller quantity of blood than natural contained in the lungs, the inference deduced by Dr. Holland is, that the production of animal heat is in the inverse ratio of the quantity of blood exposed to oxygen. But this inference is neither logical nor sound."†

I am here shown to be opposed to the theory, that animal heat is proportionate to the chemical changes of the blood in the lungs. This is a misconception on your part, and derives support from one circumstance only, an imperfect definition heading the first chapter of the "Experimental Inquiry." The definition is, "that animal heat is not in the direct ratio of the quantity of oxygen inhaled, but in the inverse ratio of the quantity of blood exposed to this principle." This definition is shown, in my subsequent works to be exceedingly faulty, not conveying to others the views then entertained by me, and which are clearly laid down throughout the whole of the "Experiment Inquiry." The imperfection of the definition arose from my affixing a meaning to the word "inhaled," which was not understood by others in the sense in which it was intended. The experiments of others, as well as my own investigations, appeared to establish, that in certain conditions of the lungs, there is a diminution of animal heat, and yet a considerable quantity of oxygen disappears in the process of respiration. The definition was therefore intended to convey this meaning, that the quantity of oxygen which disappears is no exact criterion of the extent of the chemical changes effected in the blood; and not, indeed, that animal heat is not proportionate to the chemical changes in the lungs. The whole volume, comprising between five and six hundred pages, is entirely devoted to the demonstration of this fact. The modifications of the circulation, in almost every possible condition of the animal system, are examined by me in relation to the co-existing power of generating animal heat, and the conclusion arrived at, in every instance, is, *that animal heat is proportionate to the chemical changes effected in the blood.*

Such being the case, as I shall abundantly prove by reference to the work itself, you may judge my surprise, in reading your strictures on a theory which you attribute to me, but which exists in no part of my writings, and, moreover, to find that this same theory is shown to be "neither logical nor sound," on principles pervading every page of the "Experimental Inquiry." It is never, perhaps, agreeable to be detected in error by the strong and clear arguments of others, but to be made appear illogical and unsound by our own arguments, wielded by others, is anything but pleasant. To prove that you have misinterpreted and misstated my views, and employed, in their imagined refutation, principles, unequivocally stated in the "Experimental Inquiry;" the facts in support of these charges, shall, for the present be taken only from this work.

Fact I., in the introduction to the work, page xx, the following passage occurs:—

"By taking into consideration, that a small quantity only of the air within the lungs is at any one moment deteriorated, and, still further, that the left ventricle contracts 70 or 80 times per minute, in order to propel the arterial blood which is transmitted by the lungs, we shall have reasons sufficiently ample to account for the possibility of these organs bearing such changes, and for the ease with which the system is supported in an equable temperature. If the body be supposed to possess thirty pounds of blood, and the heart to transmit at each contraction two ounces, and to contract 75 times per minute, we shall find that the whole mass of blood will pass through the lungs once every three minutes, or twenty times per hour. As it has been proved by direct experiment that the blood acquires at least one degree of heat in passing through the lungs, it necessarily follows, at this moderate calculation, that the system will receive 20 degrees of heat in an hour, or 240 degrees every twelve hours. If the respiration be accelerated, and the contractions of the heart be increased to 100, the mass of the blood will circulate through the

† "The Philosophy of Health," p. 142.

thoracic organs in one-fourth less time than is stated above, and, consequently, the temperature will be augmented one-fourth; the increase of *one* degree, instead of being repeated every *three* minutes, will be repeated every two minutes and a quarter."

Is it possible to lay down the chemical theory in more distinct terms than in this passage, which forms a part of the introduction to the "Experimental Inquiry"? It is unequivocally stated, that the chemical changes are proportionate to the frequency with which the mass of blood circulates through the lungs.

Fact II. "And lastly, the petechiæ, maculæ, or vibices, express more eloquently than words can describe, the impoverished properties of the blood, and the cause to which these changes must be attributed, viz.—the congested state of the lungs and internal organs, on which account the blood is imperfectly oxygenated, because the inspired air has to act on a quantity much greater than circulates in the pulmonary cells, during inflammatory affections, moderate exercise, or health."—P. 19.

Fact III. The following is laid down as a law, founded on foregoing investigations:—"The buffy or inflammatory coat of blood in diseases and exercise, will be proportionate, *cæteris paribus*, to the quantity and force of blood circulating through the lungs, as its changes bear a ratio to those conditions."

Can any statement be more explicit than this, in favour of the chemical theory?—P. 21.

Fact IV. First: "Animal heat is proportionate to the capacity and activity of the lungs, not understanding by activity the number of respirations, but the number and character of its compounded acts, inspiration and expiration.

Secondly: "This principle (animal heat), is proportionate to the chemical changes in the lungs."—P. 34.

Can words be found to express more clearly the chemical theory?

Fact V. "If the circulation of blood in the lungs be impeded by an increased quantity being determined to them, as by the depressing passions of the mind—by the paroxysm of spasmodic asthma, nausea—cold and soporific medicines, the evolution of heat is proportionately lessened."—P. 36.

"The buffy coat is seldom or never present without having been preceded or accompanied by an augmentation of animal temperature: in fine, its extent is generally observed to bear a ratio to the increased temperature, and this as invariably is proportionate to an excited state of the respiration and circulation."—P. 36.

Here, again, it is stated distinctly, that animal heat is proportionate to the chemical changes of the blood.

Fact VI. "The influence of warmth in animals operates upon the surface of the body, and the organs which are fitted to improve the qualities of the blood. By exciting the surface, the action of the capillaries is universally stimulated; and the consequence of this is, that the internal organs participate in the change, as they are necessarily relieved of a portion of blood, from its more equal diffusion, and are rendered more capable of oxygenating, transmitting, and assimilating that which they receive."—P. 117.

Fact VII. "After fatigue and perspiration, the blood is transmitted with less vigour, and less stimulating properties, to the surface, on account of the diminished action of the lungs and heart; and, therefore, if cold be recommended in this state of the system, it will disappoint the expectation of the physician, because it will determine the blood internally, and control still more the weakened function of the lungs and heart, so that the oxygenation and the propulsion of the blood will be much impeded."

Fact VIII. "The blood is driven upon the thoracic and abdominal organs, and destroys, in a short time, the balance existing between the internal and external circulation of blood. The lungs receiving more than they usually possess, the additional quantity necessarily impedes the oxygenation and circulation of the blood; and in proportion as these essential conditions are disturbed, we shall find that the faculty of generating animal heat is diminished."

Fact IX. Speaking of the effects of disease, it is stated:—

"The increased frequency of respiration, and the preponderance of its expiratory character, are to be regarded as the principle causes in augmenting the evolution of heat. Before disorganisations occur, the blood circulates with greater velocity through the lungs; the constant or frequent cough prevents or retards its occasional accumulation; and from these conditions the sanguineous fluid is more completely oxygenated."—P. 191.

Fact X. "I have proved by direct experiment on myself, and on another gentleman, that whenever vomiting is promoted, the blood is immediately more equally distributed throughout the system, and, as a necessary consequence, that the temperature of the body is increased."—P. 252.

Fact XI. "Depletion has been found useful in the cold stage of intermittent fever, because it takes off a part of the burden of the internal organs, and thereby permits the lungs to oxygenate more completely the blood, which speedily excites the action of the heart, the increased contractions of which distribute the blood more generally throughout the system."—P. 153.

Fact XII. "At this period the face and extremities have less blood than in the state of health; the temperature of the body is

also diminished, because the increased quantity in the chest lessens the capabilities of the lungs to produce the ordinary effects upon it."—P. 310.

Fact XIII. "The frequent respirations, supposing other conditions of the system favourable, as they are almost universally in childhood, must be allowed to deteriorate, in a given time, a greater quantity of air than when these are slow or imperfect; and if a greater quantity of air be breathed in a limited period, a greater quantity of oxygen must be received by the blood in the lungs in the same time, and it is to this agent we must refer the oxygenated or arterialised properties of the sanguineous fluid."—P. 384.

"Is it possible to lay down a more distinct statement in favour of animal heat, *being in proportion to the chemical changes of the blood in the lungs*," than is contained in this passage?

Fact XIV. "Those also observed in the first chapter, that whenever the lungs possess a quantity of blood greater than what is natural, the chemical changes are materially diminished. The influence of grief prevents the conditions essential to a perfect oxygenation of the blood, as it brings to the lungs an inordinate quantity of it, and, therefore, increases on the one hand the blood that has to undergo the chemical changes, without augmenting on the other, to the same extent, the proportion of air within the lungs to produce these effects."

The above extracts are copied from the "Experimental Inquiry," the work to which you allude in your strictures, and it would not be difficult to present a hundred other passages, clearly corroborating the same physiological principles, but it is not necessary to multiply them. Those already given are sufficient to show, that animal heat is regarded by me as proportionate to the chemical changes effected in the blood in the lungs. We will take now a few passages from works published by me since the "Experimental Inquiry," which appeared in 1829, and if any part of the extracts be in italics, understand that they are so in the work whence they are taken. The first work we will examine is, "The Physiology of the Fœtus, Liver, and Spleen," published in 1831.

In treating the subjects of this work, it was not necessary to say much on any theory of animal heat; the following passages, however, are not less explicit than those already given.

Fact XV. The following are laid down as laws, at the end of the chapter, in which the "Primary and Secondary Functions of Life" are discussed.

"4. That the ascending sap in plants is similar to the chyle in animals, unfit for nourishment until it has been acted upon by atmospheric air; and that the descending sap, from the nutritive properties it possesses, bears a striking resemblance to the arterial blood, the great source of vital actions.

"5. That the animal system cannot derive nourishment from any substance, unless it passes through the lungs; and still further, that the extent and perfection of assimilation will, in a great measure, be regulated by the extent and perfection of the chemical changes which the blood undergoes in the respiratory organs.

"6. It is also manifest, that there are no organs of the body, except the lungs which can, under any circumstances, produce chemical changes in the circulating fluid, any more than the bare trunk is capable of effecting in the tree those similar changes, which in summer are produced by its numerous leaves. The lungs are, therefore, to be regarded as the only cause of animal heat, and of those chemical changes which the blood undergoes in passing from venous to arterial."

Fact XVI. "It is stated in the 'Experimental Inquiry into the Laws of Organic and Animal Life,' that the blood, in all inflammatory affections, and under any excitement of the system by exercise, and stimulants of every kind, is better oxygenated from its more frequent circulation in the lungs."

Fact XVII. "The more or less perfect manner in which oxygen is enabled to act, occasions all the diversities in the colour of the circulating fluid in warm-blooded animals, at different ages, and, also, all the varieties in the degrees of animal heat."

We will pass now to the examination of the "Inquiry into the Principles and Practice of Medicine," vol. i., published in 1834.

Fact XVIII. The following definition heads chapter v., page 317, and there is also a note appended to it which is given with it here.—"Animal heat is proportionate to the changes produced in the properties of the blood in the lungs, and these changes are invariably in the inverse ratio of the quantity of blood circulating in the lungs at the same time." "The following definition of animal heat is given in the 'Experimental Inquiry into the Laws of Life':—Animal heat is not in the direct ratio of the quantity of oxygen inhaled, but in the inverse ratio of the quantity of blood exposed to this principle." This definition is very faulty; it does not express the exact views I entertained, and which, indeed, are clearly stated in many passages of the "Experimental Inquiry." In all my experiments and observations it seemed pretty evident that the less the quantity of blood existing, at any one moment, in the lungs, the greater was the production of animal heat; and the greater the quantity of blood, the less was the production of animal heat; it ought, therefore, to have been stated in the defini-

tion, as it is distinctly afterwards in the body of the work, that the chemical changes in the former condition are greater than in the latter, in consequence of a greater quantity of oxygen being converted into carbonic acid gas; but instead of this meaning being clearly conveyed, the converse of it appears to be stated."

The definition, and its accompanying note can leave no doubt as to my views on the subject of animal heat. They are still further enforced and illustrated in pages 309, 312, 319, 320, and 327; and, in treating of them in this work, a constant reference is made to the "Experimental Inquiry," as containing their more complete and satisfactory development. There is one other passage I will present.

Fact XIX. "7. It frequently happens that there is an increased production of animal heat, in cases of internal congestion, in which there is no evidence of the existence of inflammation, and the phenomenon is not difficult to explain. The respiratory function is excited to greater action, *so that, in a given time, a quantity of air is inspired, greater than what is natural, which cannot fail*, although the lungs are unfavourably circumstanced for *extensive* chemical changes, to confer additional oxygenated properties on the blood."

The passage which is printed in italics, is so explicit, it might have been copied from your own work. The above extracts are taken from two works, one of which has been six, and the other nearly four years before the public.

I proceed now to show that the arguments you employ in refutation of my supposed theory, exist almost verbatim, in my works alluded to.

The following are your observations:—

"If, as a comparison of all the phenomena of respiration exhibited throughout the entire range of the animal kingdom, shows, the production of animal heat be in the direct ratio of the quantities of air and blood, which are brought into contact, and which react on each other, every phenomenon of respiration must be in harmony with this law, and, accordingly, when really understood, it is found to be so.

"Inspiration, by the dilatation of the thorax, and consequently of the lungs incident to that action, is favourable to the flow of blood to the lungs. But it is only a certain degree of dilatation of the lungs that is favourable to the flow of blood through them. If the dilatation be carried beyond a certain point, the quantity of blood transmitted through the pulmonary tissue is diminished; if the dilatation be carried further, the transmission of the blood may be wholly stopped. *The quantity of the blood which flows to the lungs, and the quantity which circulates through them, are not then identical.* So large a quantity may flow to them, as to impede, or retard, or wholly stop the pulmonary circulation. In proportion to the accumulation of blood in the lung must necessarily be the distention of the pulmonary tissue; in that proportion the lung must be approximated to its condition in the experiment in which it was distended with water, when it did not transmit a single particle of blood. Further, in proportion to the preternatural distention of the pulmonary tissue with blood, must be the exclusion of air from the air vesicles, for the lungs can contain only a certain quantity of blood and air, so that the blood can preponderate only by the exclusion of the air.

"In those states of the system, then, in which the preponderance of the inspirations induces a preternatural accumulation of blood in the lungs, the production of animal heat is diminished for a two-fold reason; first, because the distention of the pulmonary tissue with blood, retards the pulmonary circulation, and proportionately lessens the quantity of blood which is brought into contact with the air; and, secondly, because the distended blood vessels compress the air vesicles, and so diminish the quantity of air which is brought into contact with the blood.

"It follows that the diminution of temperature which takes place in this condition of the system, is not because the production of animal heat is in the inverse ratio of the quantity of blood which is exposed to oxygen; but because, from a twofold operation, there is a diminution of the quantity of blood and of oxygen, which are brought into contact."

We will examine the important passages in this extract, and to be as methodical as possible, we will number them, and place against each, a few parallel passages from my works.

Dr. Southwood Smith's Passages.

(*a*)—A comparison of all the phenomena of respiration, exhibited throughout the intire range of the animal kingdom, *shows the production of animal heat to be in the direct ratio of the quantities of air and blood which are brought into contact.*

Dr. Holland's Passages.

(*a*)—First. "*The more or less perfect manner in which oxygen is enabled to act, occasions all the varieties in the degrees of animal heat.*"—*Phys. of the Fœtus*, p. 101.

Second. "*Animal heat is proportionate to the chemical changes in the lungs.*"—*Experimental Inquiry*, p. 24.

Third. "The frequent respirations, supposing other conditions of the system favourable, must be allowed to deteriorate, in a given time, a greater quantity of air than when these are slow or imperfect."—*Experimental Inquiry*, p. 384.

Fourth. "The respiratory function is excited to greater action, so that, in a given time, a quantity of air is inspired, greater

than what is natural, which cannot fail to confer additional oxygenated properties on the blood."—*Inquiry into the Principles and Practice of Physic*, p. 324.

Fifth. "If the respirations be accelerated, and the contraction of the heart increased to a hundred, all other conditions being equal, *the mass will circulate through the lungs in one-fourth of the time, and the production of animal heat will be augmented in the same proportion.*"—*Experimental Inquiry, Introduction*, p. xxi. *Inquiry into the Principles and Practice of Physic*. p. 319.

[In these extracts, the principle you contend for, is most distinctly laid down. The statement is so clear it cannot admit of misconception, and it has now been before the world almost ten years.]

Dr. Southwood Smith's Passages.

(*b*)—*The quantity of the blood which flows to the lungs, and the quantity which circulates through them, are not then identical.*

Dr. Holland's Passages.

(*b*)—Almost the half of my life has been spent in endeavouring to establish this difference, and to shew the importance of its application to medical science. What do the following passages say?

First. "*If the circulation of blood in the lungs be impeded by an increased quantity being determined to them*, as by the depressing passions of the mind, by the paroxysms of spasmodic asthma, nausea, cold, and soporific medicines, the evolution of heat is proportionately lessened." — *Experimental Inquiry*. p. 36.

Second. "The lungs receiving more blood than they usually possess, the additional quantity necessarily impedes the oxygenation and circulation of the blood, and, in proportion as these essential conditions are disturbed, the faculty of generating heat is diminished."—*Experimental Inquiry*, p. 128.

Third. "Depletion has been found useful in the cold stage of intermittent fever, because it takes off a part of the burden from the internal organs, and thereby permits the lungs to oxygenate more completely the blood."—*Experimental Inquiry*, p. 253.

Fourth. "At this period, the face and extremities have less blood than in the state of health, the temperature of the body is, also, diminished, because the increased quantity of blood in the chest, lessens the capabilities of the lungs to produce the ordinary effects upon it."—*Experimental Inquiry*, p. 310.

Fifth. "Depressing passions have one universal tendency, to augment the proportion of blood which the lungs receive, and therefore to deteriorate the condition of the sanguineous fluid." — *Experimental Inquiry*, p. 353.

Sixth. "The influence of grief prevents the conditions essential to a perfect oxygenation of the blood, as it brings to the lungs an inordinate quantity of it, and therefore increases, on the one hand, the blood that has to undergo chemical changes, without augmenting, on the other, to the same extent, the proportion of air within the lungs to produce these effects."—*Experimental Inquiry*, p. 389.

Seventh. "Strong muscular exercise always occasions a quicker circulation, a more equable distribution of the blood, and an augmentation of animal heat. It cannot, however, produce these effects, in any other way, than by diminishing the quantity of blood existing at any one moment in the lungs, and by accelerating its motion through them."—*Inquiry into the Principles and Practice of Physic*, p. 310.

Eighth. "In a congestive state of the lungs, as indicated by imperfect or difficult breathing, a feeble and frequent pulse, and great susceptibility of cold, the whole of the blood does not circulate through them so frequently in any given time, as when they are in a healthy state, and the inspired air, moreover, does not act upon it to the usual extent."

Ninth. "Besides retarded circulation, which in part accounts for the diminution of temperature, the engorgement of the lungs very much circumscribes the action of the inspired air, and consequently renders it less efficient."

Tenth. "It is not strictly the province of the physiologist to show in what way heat is evolved from the combination of different gases, this being more properly the business of the chemist. Should it hereafter be discovered, that animal heat is altogether disengaged in the capillary system, or is the production of nervous influence, the more important of the practical principles, which are laid down in this treatise, will be little affected by the circumstance, because they are founded on conditions of the system, easily to be appreciated, which will always be found to exist in conjunction with the phenomena specified, whatever theory of animal heat be proposed for our adoption."—*Inquiry into the Principles and Practice of Physic*, p. 321.

The rest of your extract suggests no further remarks, being only an illustration of the two principles already discussed, which are clearly identical with my own. You are pleased to apply to me the epithet ingenious; but, if on the supposition of being the author of the theory which you satisfactorily prove to be "neither logical nor sound," I am afraid I must resign all claim to it. I have endeavoured to establish, as briefly as the subject would allow, the three propositions at the commencement of this paper.

First,—To show that you have altogether misinterpreted and misstated my views.

Second,—To show, by reference to my works published subsequently to the "Experimental Inquiry," that the chemical theory of animal heat is stated in the most un-

equivocal terms, viz., *that animal heat is proportionate to the chemical changes which the blood undergoes in the lungs.* And,

Third,—To prove, that the principles adduced in refutation of my supposed theory, are either distinctly stated or illustrated, in almost every page of the "Experimental Inquiry."

If, in these remarks, an expression have escaped me, in any degree wanting in courtesy, or good feeling, you may rest assured it is not in harmony with the prevailing sentiments of my mind, nor is the occasion of addressing you altogether painful, because it allows me to offer my humble meed of praise in approbation of your labours, and though personally unknown to you, to assure you, I remain with respect, your obedient servant,

G. CALVERT HOLLAND,
Physician to the Sheffield General Infirmary, and Lecturer on Physiology, and on Medical Jurisprudence in the Sheffield Medical Institution.

Sheffield, Jan. 10, 1838.

PROFESSOR GRAHAM.

To the Editor of THE LANCET.

SIR:—In a late LANCET a "Student of *University College*" strongly complains of Professor Graham's want of elocution and distinct enunciation. Now, I willingly admit that Mr. Graham's Scotch brogue was, *at first*, rather unintelligible to a large body of his class, but since his appointment the students have naturally become more accustomed to his broad accent; and he is himself, moreover, wonderfully improved, being much more distinct and energetic in his mode of lecturing; for you must consider that the theatre in which he lectures is one of the largest in London, capable of containing from 800 to 1000 people, and it would be by no means an easy task, even for a man possessed of "Stentorian lungs," to be heard in every corner. * * *

In conclusion, let me bear my humble testimony to Professor Graham's urbanity of manners, and persevering attention to his class; for he generally stops *half an hour at least after lecture*, to explain any subject that the student may desire, and which, of course, must prove highly advantageous to his numerous auditors. I remain, Sir, your obedient servant,

FAIRPLAY.

London, Jan. 1, 1838.

THE LANCET.

London, Saturday, January 20th, 1838.

WE have recurred so frequently to, and dwelt on, at such length, in the pages of this Journal, the manifold abuses of the system of medical instruction recognised in this country, that it is difficult to discover any new enormities which are left for us to expose and denounce. Our Medical Corporations, however, represent so many beds of corruption, in which rank weeds spring up with surprising fertility, that if we now return to evils which have many a time exercised our pen, in preference to new ones, it is because the public mind is familiarised with their magnitude, and therefore most eager for their reform. We return to them at this period, especially, when the curriculum of the *London University* is most probably on the eve of being issued.

We feel that our duty of exposing corrupt practices, at all times a solemn and a noble one, becomes, at this moment, incalculably more serious than ever, when the first University founded on liberal principles is about to publish its laws. Our first topic—our favourite one for attack, because it is the strong hold of the enemy—is the CERTIFICATE SYSTEM, that incarnation of all which is absurd in principle, abortive and useless in practice, and foully dishonest in object. *Absurd* in principle: for what matters it where knowledge is acquired, provided it be comprehensive and sufficient? Is there a virtue in some stone walls above others? Does learning alight on a man because he has walked certain hospitals, where eel-backed Pures lisp forth their prescriptions, and worthless surgeons sacrifice the lives of their fellow-creatures through their ignorance of pathology, or their barefaced neglect of duty? Nay, more, is it not notorious, can it be denied even by the patrons of certificates, that foreign universities afford instruction, in many branches of medical instruction, which is immensely superior to that which our own now yield?

We may instance the subjects of practical anatomy, morbid anatomy, operative surgery, midwifery, and diseases of women and children.

Would not this truth force a conviction of the necessity and justice of altering our system on any minds save those of the sordid hirelings and chiefs in our dens of medical peculation. "Oh, but," say the old gentlewomen who pocket fees either in their own persons or in those of their minions, "we want our "young men to learn the practice of the London Hospitals!" Did we not *know* the clique, we should fancy the old ladies were jesting with us. What! do they mean that the pupil should acquire an indifference for science, recklessness as regards human life, negligence of duty, the art of promising amendment every October, and regularly breaking faith with a look of premeditated insult to the dupes whom they may have caught? If so, we admit, by all means, that they should send candidates for their bit of paper to the London Hospitals; for in them, indeed, such qualifications may be studied from the life. But if it be desirable that our young men should become useful members of their profession by the acquirement of correct knowledge, regular habits, and honourable dealing, *we* bid them study at the Antipodes rather than in the London haunts of chartered knavery.

Even at Edinburgh, where, as we learn, tickets are daily taken at the doors, the hour of lecture-drudgery is constantly shirked. A graduate of the University of that town has informed us, that during his stay there he *absented himself completely during twenty lectures out of one course*, and, with a few exceptions, never entered the room until twenty minutes or half an hour after the beginning of the lecture; yet, at the end of the session, he got a certificate of having attended "*very regularly.*" Now, by a late regulation, these words meant that he had not been absent on *any two occasions* during the entire course! But the worthy lecturers have door-keepers, and there are ways and means of propitiating those high functionaries. Like master, like man, with this exception, that the latter sells you, *at your own desire*, what, as things exist, is valuable, while the former *forces* you to pay for his worthless twaddle which you despise.

When the lecturers are avowedly worthy of being heard there is no need of force to muster an audience. Look at the case of the Parisian professors. Is it not notorious that their doors are besieged by the pupils for a length of time before they are opened, and that such is the anxiety to hear an ORFILA, a RICHARD, or an ANDRAL, that the Faculty has been obliged to issue orders that pupils of a certain standing only should be admitted to their lectures. When will such men be found among *our* medical teachers? When will such ardour be evinced by our students? Senate of the *London University*, it depends on you. Perhaps, even before the present generation passes away, if you discharge the sacred trust now reposed in you with judgment and honour. Who shall say when, if you dishonour your high office by yielding to the petty interests of the hirelings who are on the alert to tamper with your better feelings?

What is the object of the "certificate system," if it be not to fill the pockets of incapable boobies who have interest enough to get themselves licensed and recognised to be, what no human ingenuity could ever make them, teachers of a science. Teachers at whose unblushing ignorance the merest tyros are only restrained from laughing by the dread of a refusal of certificates. It was well said that there is no royal road to learning; meaning, that the only mode of mastering a science was the legitimate one of labour and perseverance. Little did the philosopher who said so know of the high roads of our times, nicely Macadamised for plebeian as well as for aristocratic noodles. What could he have dreamed of the marvellous shop in Blackfriars, where experienced chemists, anatomists, botanists, &c., are "made to order on the shortest notice."

We are told that some swarthy mulattoes, in the West Indian Isles, cannot, from their

depth of colour, wholly succeed in the great object of their ambition—getting themselves whitened by law. The authorities, however, compromise the matter, and pocket the fees, by a dubious verdict—"*Que se tengan por blancos.*" The venality of our drug-vending judges goes a little farther; there is nothing implying doubt in their decree.

Senate of the *London University*, annihilate this unworthy system of certificate-mongering;—encourage the establishment of the *Concours*, as it exists in France, the sole means of clearing out the Augean stable of our schools, of getting workers instead of idlers, of securing the services of men who will honour, not disgrace, the institution which you are called upon to govern. Do this and you strike at the root of all the evils under which scientific medicine groans in England. Why is it that we hear such frequent complaints against the oratorical deficiencies of our lecturers? Because they have never accustomed themselves to speak in public until the moment at which their new-fledged genius descended on their heads in the form of a permit from Apothecaries' Hall. Englishmen are not a whit more deficient in natural talent for public speaking than the French. Our House of Commons, where a stimulus for its exercise exists, triumphantly proves this. Create this stimulus among our medical teachers by encouraging the establishment of the *concours*; familiarise our medical youth with the habit of expressing their ideas in public; and, beginning with the beginning, advocate the giving of hospital dresserships to persons who, among other qualifications, can speak their own language before strangers, not to individuals who, in the coarse slang of our hospital harpies, can "come down most freely with the blunt." Do this, and you will soon see *lecturers* in our metropolitan schools.

Do away with certificates, and you at once secure a new system among our hospital physicians and surgeons. All will be changed: you will have activity instead of sloth; care, instead of slovenliness; regular attendance, instead of random visits; while the pains now taken in escaping from the delivery of lectures will be employed in preparing them. But how, say you, will such results spring from the abolition of certificates? Simply by its putting the home and foreign hospitals on fair grounds of competition. The loaves and fishes of our town monopolists will then be in danger, unless they mend their ways. We should not be surprised to find that their zeal in effecting improvements then become as great (greater it cannot be) as it now is in aiding and abetting every sort of abuse.

But if there be one point in which above another it behoves the University Senate to alter the existing state of things, it is in that of Examinations. As regards this evil, we cannot possibly fear the result. If they have inquired, as they should have done, into the working of the present method of ascertaining the fitness of candidates for medical practice; if they have made themselves acquainted with the scenes of tyranny and oppression which disgrace the torture-room; with the ruthless manner in which a power is exercised against which there is no appeal; if they have acquired a just notion of the extent of ignorance of the imbecile Examiners; and if they have not, it is their own fault, for we have given them ample opportunities for informing themselves on these points;—then, we say, they MUST make the Examinations public. The public have a right to know the capability of the judges to question, as well as of the candidate to answer.

By giving publicity to the inquiry, too, its nature will be changed altogether. It will become practical and sound, instead of theoretical and flimsy. We shall no longer meet "distinguished medallists," who will tell us, with ease, the mean number of hairs, black, red, or brown, that grow on a square inch of scalp, but have not an idea of the position of the chief vessels of the body; who will overwhelm us with theories on the growth of the ovule, but cannot distinguish

a holly tree from an oak; nor, on the other hand, shall we have young men turned back, as they now frequently are, by irresponsible despots, for not answering "catch questions," which, in nine cases out of ten, it would be a disgrace to them to comprehend. We shall have young men, too, sent forth to guard the public health, who can prove their capability of distinguishing one disease from another at the bed-side of the patient, and their knowledge of anatomy by demonstrations on the subject.

For the present we conclude. We shall return to these questions, and enter more into the detail of the changes which we, and all friends of justice, desire. Meanwhile we earnestly beseech the members of the Senate to weigh well the importance of the charge reposed in them. Let them be assured, that if they, too, yield to the intrigues of the crafty monopolists, the contempt of all good men will fall on them; the medical public will ransack every nook and cranny of their rotten edifice, for infamy will be exposed wherever it exists. But let them discharge their duty as they ought; let them make just and rightful regulations; let them convert our lecturers from brazen pilferers into hard-working public servants; let them raise our national medical character from the contempt into which our corrupt institutions have sunk it in the eyes of Continental Europe: all this they *can* do, and the gratitude of an enlightened public will repay them for their efforts. Let them look on this picture and on that, and can they hesitate which to choose? We trust not; we think not. But, if with a lamentable perversion they strive to perpetuate abuse, all the punishment we wish them is, that they may see others achieve the noble task of reform which they were base enough to refuse; that they may suffer under, what philosophers tell us is the worst of pangs, the remembrance of having committed a bad action in vain.

We have been asked to furnish, in The Lancet, some explanations respecting a medical return lately moved for by Mr. Wakley in the House of Commons, and now in circulation among the medical officers of the new Parochial Unions. The particulars of that return appear to us to be sufficiently explicit, and will be generally understood; we shall, therefore, confine our observations to a few remarks which may be useful to some medical gentlemen, when filling up the returns. The return is for the year 1837, and is intended to show accurately the degree of labour performed by the medical officers of Unions during that period, and the sums of money which they respectively received by way of "remuneration." It is, of course, of great importance, that the amount of sickness and attendance should neither be exaggerated nor understated. For this reason the table has been so constructed that one part checks another; and any error may be easily detected by a comparison of one part of the return with the other, or by a comparison of the return made by different persons. The medical man's name will, by order of the Poor-Law Commissioners, be signed at the bottom of each schedule, will be published, and a surplus number of the returns will be circulated all over the kingdom, so that any error arising from negligence or ignorance will easily be traced to its source.

The first items in the return require scarcely any remark; but it may be mentioned, that the area of the district should be given in square *miles*; and that the age is always to be understood as that which was attained at the last birth-day.

The "Age of the medical officers on taking office," implies, "under the Poor-Law Guardians." The "age on entering district" is synonymous with the "Age on entering the district as a medical practitioner." A correspondent who appears to understand the latter question, notwithstanding, writes to ask whether it is synonymous with the question preceding,—a doctrine which is held by the clerk of the Union. We reply, *No:*

The first is the age on originally taking office under the Poor-Law Guardians in the district; the second is the age attained on commencing practice in the district,—two things which are perfectly distinct.

The average number constantly on the sick-list may be determined by taking out the numbers known to be sick, and under the care of the medical officer, on the last day of every week, placing them in a column under each other, adding the whole together, and dividing, where this has been done through the year, by 52; or the mean number "brought over" weekly in the parochial book of attendances, will be an answer to this question. The "total number of cases treated" does not include those under treatment, Dec. 31, 1836, but the total number of new cases that applied for relief in 1837. It has been found in benefit societies that the same person is often treated twice or three times in the year; and thus contributes two or three cases to the general list of attacks. Where, out of 100 members, 32 different *persons* had attacks of sickness, the attacks themselves —the cases—amounted to 40. In the "total number of cases treated" a man's name will be counted every time he has a distinct attack; in the "total number of persons treated" the same individual will never be counted more than once in the year. None should be entered as "cured" that were not positively known to be cured by the surgeon. The list, therefore, will not include all the recoveries, as many cases are lost sight of towards their close.

DR. JAMES JOHNSON AND DR. DICKSON.

To the Editor of THE LANCET.

SIR:—At the moment you closed the columns of your Journal against further further controversy between Dr. Dickson and myself, my adversary had the advantage of both the first and the last words; or three attacks against two defences. This was hardly just in the umpire of a literary war. The matter was not mended when the columns of THE LANCET were, *a fourth time*, just opened wide enough to let in a jet, or the sting of a libel, from Dr. Dickson, in the shape of a postscript from Widow Gibbon, averring that I took fees from her late husband. She does not, indeed, assert that I knew the profession of Mr. Gibbon; but Dr. Dickson, with his usual candour and accuracy, makes up the defect, by stating that *I myself acknowledged my cognizance of Mr. G.'s medical avocation!* Now, your readers must be aware, that, in my second and last note, I distinctly disclaimed all recollection of Mr. Gibbon, and that I had searched my visiting-book for 1835, without finding his name—hence I concluded that, if Mr. G. did consult me at all, it was at my own house, with other patients, whose professions or avocations I knew not. To Dr. Stewart, of Portsmouth, indeed, a DR. Gibbon confessed his obligations to me, for attendance without fees, several years ago, when on furlough for his health; and, admitting that this is the same individual as Mr. Gibbon, the subject of debate, is it likely that I would, at one time, give gratuitous advice to Mr. or Dr. Gibbon, and, at another time, take fees from him, knowing him to be a brother practitioner? None but an illiberal adversary would draw such a conclusion. But why rake up the ashes of the dead for unsatisfactory evidence, when so many hundred medical men are now living in England, who can certify to the fact of my taking fees—*if it be a fact*. If I have done so, conscious of their profession, let them come forward freely, and condemn me. They owe me no obligations under such circumstances, and I shall not upbraid them for exposing my conduct. There are a hundred, if not more, in London alone, to whom Dr. Dickson may apply for evidence to confound me. I shall just give him a short list of those whom I, at this moment, recollect:—

Dr. Clendinning, Wimpole-street.
Sir A. Brooke Faulkener (late of Cheltenham), Alpha Cottages.
Dr. Morrison, Cavendish-square.
Dr. Donahoo (Staff), Suffolk-place.
Dr. Tweedie, Montague-place.
Dr. Beattie, Tenterden-street.
Dr. Thomson, Hermitage-place.
Dr. Jewel, Golden-square.
Dr. Davies (East India Comp. Service), Pelham Crescent, Brompton.
Dr. Scott, 369, Strand.
Mr. Gaskell, Chelsea.
Mr, Pretty, Mabledon-place.
Mr. Rowe, Woburn-place.
Mr. Chinnock, Brompton.
Jones Quain, Esq., London University (to testify my long and painful attendance on the late Dr. Bennett).
Mr. Laisné, Eaton-square.
Mr. Dickenson, Sloane-street, &c. &c. &c.

To any of the above, or fifty others whom I could name, in this metropolis alone, Dr Dickson may apply for *authentic* informa-

tion respecting my habits and character on the point of dispute. I am, Sir, your most obedient servant,

JAMES JOHNSON.

Sunday, Jan. 14, 1838.

*** The insertion of this letter demands from us the remark, that we did not intend to act, in the slightest degree, unfairly to either party, when we stated, some weeks since, that the correspondence between Dr. James Johnson and Dr. Dickson was then concluded in our columns. We have ever made it a rule, in conducting this Journal, to abstain from noticing the professional conduct of private medical practitioners, unless it have been made the subject of public comment elsewhere, or have become matter of judicial inquiry in a court of law. Experience has fully confirmed us in the conviction, that we could not have adopted a more wise or salutary resolution. In the present instance, the angry discussion between Dr. Johnson and Dr. Dickson, arose out of an attack on the private practice of Dr. Dickson, in a review of that gentleman's work, which was published in Dr. Johnson's "Quarterly Journal." The answer of Dr. Dickson, to the imputations of the Reviewer, brought with it an attack on the private professional practice of Dr. James Johnson. Thus has the crimination and the recrimination proceeded. We are decidedly of opinion that the first fault was committed by Dr. Johnson's reviewer, and that the imputation upon Dr. Dickson's integrity was wholly unwarranted, and without foundation. Equally are we of opinion that a grievous fault was committed by Dr. Dickson, when he endeavoured to justify his own professional practice by attacking that of his opponent; and since Dr. Johnson appears to be of opinion that when it was stated in THE LANCET that the correspondence could not be continued, his adversary had then "the advantage of both the first and the last words," we know of no better mode of convincing Dr. Johnson that we had not the slightest intention of placing him in an unfavourable position, than by embracing this public opportunity of bearing testimony to the liberality which he has invariably displayed in the practice of his profession. We consider Dr. Dickson to be in error in making his accusation. We do not believe that Dr. Johnson ever knowingly received a fee from a brother practitioner, and on many occasions his great liberality to non-professional patients, is a fact which has fallen under our own immediate observation.

DR. DICKSON thinks that we have acted unfairly towards him in the controversy which has recently taken place between him and his literary opponent. If he be correct in his supposition, we have committed an error of judgment, but certainly our intentions are free from culpability. Dr. DICKSON considers that we ought not to have excluded from our columns the letter from which we, last week, published a short extract, and the Doctor now requests that the communication in question, if we are still of opinion that it should be excluded from the body of the work, may be inserted on the cover of THE LANCET as an advertisement.

The letter of Dr. JOHNSON, which is inserted at page 609, of our present Number, and the remarks appended to it, were in print previously to the receipt of the note which was sent to us by Dr. DICKSON in the present week. As our opinion of the impropriety of continuing the controversy between Drs. JOHNSON and DICKSON remains unchanged, we can only state, that the excluded communication of Dr. DICKSON shall be printed as an advertisement on the cover of THE LANCET of next week. We regret that the arrangements of the printer render it impossible to include the article in the advertising columns of the present number of our Journal.

WESTMINSTER MEDICAL SCCIETY,

Saturday, January 13th, 1838.

Mr. HALE THOMSON, President.

OBSCURE ORGANIC DISEASES.—ERRORS IN DIAGNOSIS.

Dr. J. JOHNSON said, it would be well if every practitioner would candidly acknowledge the errors in diagnosis which he committed. In plainly stating the error into which he had fallen in the case he was about to relate, he might also mention that Drs. Bright and Elliotson, who had also seen the patient, had fallen into the same error. The subject of the case was a tradesman, who had been ailing during the last two years, his chief complaint being a constant unpleasant pain in the epigastric region, and gradual emaciation; he had no vomiting.

During the last eight months he had been attended by Mr. Dunn, and the prominent symptom was pain at the pit of the stomach, which was increased on taking food. There had been no vomiting, no cough, and no expectoration; he was subject to headach; his pulse was small, sharp, and rather quick, and varied from 80 to 100; there had been no jaundice, and no tinge of the skin, while under Mr. Dunn. Drs. Bright and Elliotson had come to the conclusion that there was malignant disease of the stomach. Dr. J. first saw the patient on a Sunday,—the following Sunday he died. When he (Dr. J.) saw him he was constantly vomiting up a glairy fluid, like coffee-grounds, which was occasionally tinged with blood. He (Dr. J.) also considered that malignant disease of the stomach was present. Nothing allayed the vomiting; leeches, blisters, and hydrocyanic acid were employed, but with no good result, and the patient sunk. After death it was ascertained that no organic disease of the stomach had existed; that organ, however, was generally inflamed, the inflammation not being more than of eight or ten days duration; the body was much emaciated. What, then, produced the constant pain at the pit of the stomach? On examining the gall-bladder it was found to be completely filled with three calculi, which had become attached to each other, and formed a solid mass, as large as an egg. This large gall-stone projected an inch, or an inch and a half, beyond the liver, and must have produced a tumour. It was remarkable that this had never been detected. The pressure produced by this tumour on the pyloric extremity of the stomach, the duodenum, and the ganglionic system of nerves in that neighbourhood, had caused the pain. The lungs were sound. On cutting open the pericardium, the heart, at first sight, appeared larger than natural, and on dividing it across it presented a most interesting case of concentric hypertrophy of that organ, the parietes being, at least, an inch and a half in thickness, and the ventricles remarkably diminished in calibre. It had been suspected at one time that the chest of the patient was affected, but an examination by the stethoscope failed to detect anything abnormal in any part of the thorax. The disease of the heart was no more suspected than the presence of the gall-stone. The kidneys were completely granulated. In May last the patient had diabetes, making large quantities of water, which led Dr. Bright to suspect that the kidney was affected. The urine, however, contained neither albumen nor sugar, and the secretion soon becoming normal in quantity, Dr. Bright abandoned his opinion. Latterly the urine of the patient had not been examined, but on the Sunday before he died some which was in the chamber utensil was found to be albuminous; not one of the three formidable organic diseases was suspected during the life of the patient.

Dr. Williams made some remarks on the mode of detecting concentric hypertrophy of the heart. He thought the case just related was an instance, exemplifying the aggravation of disease of other organs, by the presence of disease of the heart.

Dr. Addison thought, that in Dr. Johnson's case the kidney was diseased in the first instance, and that the other organic changes might be referable to such disease; the case was not unusual, and the symptoms described such as we occasionally observe in cases of mottled kidney.

Mr. Streeter then read a case, with remarks, entitled,—

A case of small-pox, after vaccination, in infancy, occurring in a woman, in the seventh month of her pregnancy, of a semi-confluent character; with remarks on the influence of pregnancy on the disease, and treatment of the mother; on the influence of small-pox on the fœtus in utero, and on the causes which prevent the perfect exemption of the vaccinated from variola.

The author commenced by saying, that as he should have occasion to mention instances of the failure of vaccination in preventing future attacks of small-pox, he wished, at the outset, to guard the members against the supposition that he was inclined to call into question the propriety of vaccination, or the just claim which it possessed to superiority over the practice, unfortunately not obsolete, of inoculating for the small-pox. Three years since he had placed before the Society five tables, showing the comparative mortality from small-pox, taken from the London Bills of Mortality. Each of these tables contained a period of fifteen years. The first prior to the practice of small-pox inoculation; the two next after the introduction of inoculation for small-pox, and just prior to that of vaccination; the remaining two, after vaccination had been introduced. He again placed these tables before the Society, and contented himself with observing, that they amply proved the progressive *decrease* of mortality from small-pox, with the progressive *increase* of vaccination.

1. From 1706 to 1720, in every 1000 cases of small-pox, there were deaths 78
2. 1745 to 1759 89
3. 1784 .. 1798 ..... 90 9-10ths
4. 1804 .. 1818 53
5. 1819 .. 1832 32

After these preliminary remarks he would now proceed to the more immediate subject of his paper:—

Emma Forbes, born Dec. 12, 1818, was vaccinated at the *Small-pox Hospital*, July 1, 1819. She was married Nov. 15, 1836. Mr.

Streeter was called to see her on the 8th of December, 1837, and found an eruption coming out on various parts of her body; this, from the history of the previous symptoms, he concluded would prove to be small-pox. She considered herself in the seventh month of her first pregnancy. She had four distinct vaccine marks on the left arm; had felt ill for the last five or six days, and the day before had taken a very active dose of medicine, which had operated at least a dozen times. The next day the eruption was clearly that of small-pox. The pocks were numerous, became full and prominent, partly confluent, but mostly distinct. The mouth and pharynx were much studded with eruptions, so much so, as to make the swallowing at one time difficult; the pocks mostly maturated between the 14th and 15th of December. She continues to feel the child, but experienced no uterine pains, or discharge.

On the 19th she had sufficiently recovered to amuse herself with needle-work, and continued to feel the child. On the 21st she had much uterine pain, which continued during the night, so that she sent for him (Mr. Streeter) the following morning, conceiving that her labour was coming on. The pain, however, ceased, and her pregnancy was now proceeding, without having experienced any obvious influence. The treatment consisted, in the first instance, of moderately diffusive stimulants, acting two or three times a day gently on the bowels, and an antimonial opiate at night. When the eruption had fairly come out, dilute sulphuric acid, with a small quantity of sulphate of magnesia, as a refrigerant, was substituted for the diffusive stimulus. This, with sedulous care, to prevent diarrhœa, to keep the apartment cool, and confining the diet principally to bread and milk, formed the chief treatment. Mild alteratives, instead of active purgatives, were employed at the decline of the disease.

The author had selected this case to relate to the Society, because he was not aware that any case of small-pox, after vaccination, occurring during pregnancy, had been recorded in this country. Rayer had recorded a similar case in his work on diseases of the skin; and he (Mr. Streeter) knew of two cases of a similar kind. One was then in the *Small-pox Hospital*, and one had lately occurred in the wife of a medical practitioner, the particulars of which case were known to a member of the Society. Another case had also lately occurred in the *Queen's Lying-in Hospital*.

The remarks which he had to offer on the case which he had related, were such as struck him during its treatment, and such as led him to make some inquiries and some reflections,—their results he would now communicate.

The first was, the influence of pregnancy on the safety and treatment of the disease of the mother?

We possessed records of the complication of small-pox with pregnancy.

1st. When the small-pox has been caught in the natural way.

2nd. When it had been communicated to the mother by inoculation.

A third class of cases occurred where, as in the instances he had adduced, the pregnancy occurred during an attack of small-pox after vaccination. There might possibly occur cases of pregnancy during an attack of small-pox, after small-pox, but he had never heard of an instance in point.

Of small-pox caught naturally in an advanced stage of pregnancy, he had himself seen one case. It occurred at Ripe, near Lewes, in Sussex, in December, 1822, but he had the opportunity of seeing the patient only once during the earlier period of the eruption. She died, as he was informed, undelivered, the eleventh day after the appearance of the eruption.

Dr. Leake, in his "Practical Observations on the Acute Diseases most Fatal to Women during the State of Pregnancy," gave six cases of this kind, one in the fifth month, and five in the advanced periods of pregnancy. The one at the fifth month recovered, but miscarried about six weeks afterwards. Four out of five in the later months proved fatal to the mothers. Labour came on in all the five, in two during the primary fever, and before the eruption appeared (it was one of these which recovered); in the third soon after the eruption appeared; in the fourth on the ninth day of the eruption; and in the fifth at the height of the eruption. Three of these cases occurred in the Doctor's private practice, three at the *Westminster Lying-in Hospital*. The Doctor notices the extreme rapidity of the labours in every case where the labour was natural.

Dr. Denman furnishes no case in illustration, but states, that if the child be expelled at any period of utero-gestation, during the eruptive fever of the mother, she may and often does escape; but if she passes the time of the eruptive fever, and labour comes on towards the crisis of the disease, as far as his observation extends, she will certainly die.

Dr. G. Pearson, in 1794, published observations on the existence of pregnancy with small-pox. He states that in twenty cases of natural small-pox during pregnancy, the disease proved fatal to three-fourths or four-fifths of the women. His paper, however, is chiefly valuable for the information it affords relative to the communication of small-pox to the fœtus in utero; among the cases recorded will be found seven of the mother's recovery from natural small-pox, and some where the nar-

rative renders it probable, although it is not specifically mentioned.

Dr. Davis, in his work on midwifery, has collected some illustrative cases, but he seems to have met with no such case in his own experience. In his appendix, however, he records two cases of small-pox after vaccination, occurring a few days after delivery; these cases had happened in the Doctor's practice. Among the cases mentioned by Dr. Davis, the following were chiefly worthy attention:—

1st. A case of recovery of the mother from petechial confluent small-pox, in the seventh month of pregnancy, under Mr. Cook, of Shropshire, taken from "Smellie's Cases of Midwifery."

2nd. One of small-pox in the mother, at the fourth month, which did well; and several others at early periods of gestation, are referred to, as mentioned by Mauriceau. Dr. Merriman had informed him (Mr. Streeter), of a case he had met with, in which the eruption came on in the mother the day after her delivery. It was a severe case, and she died in a few days.

The last case of this kind the author should refer to, was one of a Mrs. Ford, communicated in the "Philosophical Transactions" by Mr. J. Hunter. This patient caught small-pox naturally, in an advanced stage of pregnancy. The eruption came out on the 8th of December. Twenty-three days after she was delivered, after a severe and lingering labour, of a dead child. The child was born with an eruption on it. The case was attended by Mr. Westall, who communicated the fact to Drs. W. Hunter and Leake.

The author had now to offer some illustrative remarks on small-pox communicated by inoculation to the mother during pregnancy. Dimsdale says, pregnant women inoculated have done well, and have not miscarried. He had inoculated some who had concealed their pregnancy. Dr. Pearson relates two cases from his own experience, one in the eighth, the other in the sixth month of pregnancy; both the mothers did well. He informs us, that in about forty cases of inoculation at various periods of pregnancy, only one proved fatal to the mother, and this was a case of Sir G. Baker's. The woman was inoculated in the sixth month of her pregnancy; and died convulsed on the fourth day of the eruption, after a violent and sudden premature labour.

The author was not aware of any recorded cases, similar to the one he had communicated to the Society, except those he had alluded to. The influence, then, of small-pox on the safety of the mothers, might be said to be most fatal, when it was caught in the natural way; there were, however, instances of recovery, where labour had come on during the primary fever; few, if any, where it has supervened during the maturating fever; many, where the labour has not come on during the mother's disease, but a week or two afterwards, when the labour seems to be produced by the death of the child, or else results from the completion of the pregnancy.

Of cases produced by inoculation the influence seems to be very limited, as only in one instance did it prove fatal

Of the cases occurring after vaccination, in two the pregnancy is continuing, and in the third labour did not occur till after the decline of the eruption. In Rayer's case, already alluded to, the patient was twenty-four years of age, and in the fourth month of pregnancy. She contracted small-pox on the 6th of April; the pustules were large, distinct, and flat. She suffered little inconvenience from the attack, but on the 28th ceased to feel the child. On the 13th of May she miscarried, and the fœtus was covered with variolous pustules.

With regard to the modification of treatment required, it would ill become him, (Mr. Streeter), from the limited experience of one case, to lay down any general plan. He should only venture to suggest the avoidance of such medicines as are likely to induce uterine action; to watch, and carefully prevent the occurrence of diarrhœa, and, if the patient passes over the maturating stage, to substitute a mild alterative course for the usual drastic purgatives.

He should now come to the consideration of the influence of small-pox on the fœtus in utero.

Much difference of opinion had prevailed, whether the child would contract the disease or escape when the mother was exposed to, or became the subject of small-pox during pregnancy. Cases, said Dr. Denman, were recorded in confirmation of both opinions, and had been communicated to him by men of integrity, with the view of deciding this point; but they were contradictory to each other.

The recorded cases seemed to the author to clearly establish,—

1st. That the mother, though frequently, does not necessarily, communicate small-pox to her child; for Dr. Leake's patient, who went through an attack of natural small-pox, with a full and rather confluent eruption, miscarried about two months after, without any eruption being noticed on the child; indeed, both himself, and Dr. W. Hunter never saw the fœtus affected, till Mr. Westall invited them to see the case before alluded to.

2nd. That the fœtus might become affected with small-pox, although the mother be exempt from the disease. Dr. Jenner and Sir W. Watson had furnished instances of this.

3rd. That when born alive, from labour

coming on during the disease of the mother, the infants often failed at a period varying from one to eight days after its birth.

4th. That it may pass through the disease during the period of its uterine life, and exhibit marks of the disease at birth. This, however, was a rare occurrence; for its extreme fatality to infants after birth also holds good in the fœtal state, so that a disease capable of pitting usually destroys the child. Cases in which the child was not destroyed are recorded by Watson and Dimsdale.

5th. That the disease in the fœtus was not concurrent and contemporaneous with that of the mother, but occurred many days later, a week or ten days after that of the mother; that it mostly kills the fœtus, and the labour comes on from fourteen to thirty days after the first appearance of the eruption on the mother, or even later. As the natural small-pox was so fatal to the mothers, this accounted for the comparative small number of instances of children born with eruptions. Dr. Pearson had, however, collected more than twenty instances; Drs. Laird and Rayer each gave a case; Dr. Davis quoted a curious instance from the "Journal de Med. Chir." vol. x. p. 463, of a twin labour, after the recovery of the mother from the small-pox, where one child was still-born and marked with small-pox, while the other was born alive without any present or past traces of the disease. The effects on the child in cases of small-pox after vaccination, remain to be seen. In a case, for the particulars of which the author was indebted to Dr. Merriman, jun., and which occurred at the *Queen's Lying-in Hospital*, the mother was delivered the first day of the eruption; her own disease proved mitigated, but the infant took small-pox twelve days after birth, and died nine days after. Dr. Davis had recorded two cases, in which mitigated small-pox occurred in the mothers a few days after delivery, and where the children were immediatety vaccinated and preserved.

He (Mr. Streeter) should conclude the subject by stating his intention of vaccinating the child of his patient, if born alive, as soon as possible.

The last subject he intended to make any remarks on, was the failure of vaccination in this case. He would rather have avoided touching on a subject on which the public mind was at that moment so peculiarly sensitive; but it was one which called for the most serious consideration of our profession. It was a circumstance which interested every one of the community. An elder sister of his patient, who was also vaccinated at the *Small-pox Hospital*, visited her sister on the 14th of December. On the 30th she had an eruption of small-pox, the next day violent hæmorrhage from the uterus and the bowels took place, the worst petechial symptoms set in, and she died the 3rd of January. She was a patient of Mr. Fortescue, of Smithfield-bars, and he had the authority of that gentleman for saying, that he never saw a more malignant case.

During the past year, ninety-five out of the two hundred and fifty-one admissions to the *Small-pox Hospital*, have been cases after vaccination; it is, however, consolatory to remark, that only one of these has proved fatal, but many have been severe. Three seasons ago he (Mr. Streeter) had endeavoured to enforce on that Society, and through them on the profession at large, the necessity of re-vaccinating after the period of puberty; the large number of instances of small-pox after cow-pock, that he had seen in young women, after puberty, had led him to that belief. He had also seen an instance of modified variola after small-pox inoculation, after puberty. Since then he had met with other instances confirmatory of the view he had taken, and he might state, that out of the ninety-five cases in the *Small-pox Hospital*, there were of the age of fifteen and upwards, the enormous proportion of more than twelve to one; the number of those past puberty being eighty-eight,—those only fourteen years, or under, being about seven.

Dr. Addison had attended one of the patients alluded to by Mr. Streeter. This lady contracted small-pox at a period when she expected daily to be confined. She had been vaccinated. The small-pox was of a most confluent character, but she eventually recovered. Three weeks after recovery she was delivered of a child, healthy, well-formed, and without any kind of blemish. He believed the child had never suffered from any affection since.

Mr. Chinnock recollected two cases of measles occurring during pregnancy; in one case premature labour was brought on, but the patients both did well.

Mr. Streeter stated, that he should not feel justified in vaccinating any patient from lymph when an areola was present.

MEDICAL SOCIETY OF LONDON.

Monday, January 15, 1838.

Mr. Bryant, President.

MORBUS PEDICULARIS.

The President stated that there was, at present, a remarkable case of morbus pediculariis in *Guy's Hospital*. The subject of this terrible disease was a woman about thirty years of age, whose occupation had been that of a governess. The body was constantly covered with pediculi over its entire surface, the irritation produced by which had induced the patient to scratch herself to such an extent, that many parts

of the surface presented the appearance which is commonly observed in porrigo. On her admission she was placed in a warm bath, her clothes removed, and every precaution used to get rid of every one of the insects; but, in two hours after, being put to bed, the surface was again covered, and all attempts at removing the vermin were unsuccessful, the regeneration of them being so remarkably rapid as to set remedies at total defiance. Nothing like cysts, containing the ova of these insects, were observable.

Dr. WHITING had never heard of a case in which the insects could breed, and come to perfection in two hours, as stated in this case. He thought there must be something obscure in it. He never met with an instance in which the insects were not removed by thoroughly cleansing the body, and bathing it with a mixture of turpentine and infusion of tobacco, of a properly regulated strength. Regarding the generation of insects, much depended on the state of the constitution; at least this was the fact in regard to cattle; poor and lean animals being generally infested, while the fatter and more healthy were free.

Mr. CRISP had seen fat oxen covered with insects; perhaps resulting from their contact with poor and lean animals. A mixture of tobacco-water and urine was generally used by farmers as a successful remedy in this disease.

Mr. DENDY said the disease under consideration was a formidable one. He believed that one of the kings of England fell a victim to it, as did also, according to report, one of the late royal duchesses. No doubt everything, in the last case, was tried, but without success. He considered that there was a peculiar state of the system of persons affected with this disease. The state of their skin might be particularly favourable to the generation of these insects, in the same way as one soil is often found more congenial to the production of certain vegetables than another.

Some conversation took place regarding the means of destroying insects, which are generally found about the pubis. The President related a case in which a young gentleman used a strong infusion of tobacco for this purpose, and nearly fell a victim to the experiment. Mr. Linacre had found the best substitute for greasy substances, to be a strong solution of soda.

PHRENOLOGIGAL SOCIETY OF LONDON.

BLIND TRAVELLER.—ORGAN OF LOCALITY.

At the second meeting of this Society for the present session, held at the Society's Rooms, 75, Newman-street, Dr. ELLIOTSON, the President, in the chair, a cast of the head of Philip Davis, a blind traveller, was presented to the Society, and a paper, containing an account of Davis, was read by Dr. JOSEPH MOORE.

Philip Davis, aged 41, a native of Plymouth, when six years of age had severe ophthalmia, which terminated in complete staphyloma. At twelve years of age he was admitted into the Asylum for Indigent Blind Persons, at Bristol, where he remained during five years, receiving the instruction which is usually supplied to its inmates; and, finally, he learnt the art of basket-making. He then returned to Plymouth. An attempt was made soon after this to form an artificial pupil, which so far succeeded as to enable Davis to distinguish large objects when moving before him. Thus he could distinguish between a man and a horse, &c.; but an accidental blow from a stone destroyed this power about two months after. He was shortly afterwards engaged by the contractor who supplied Plymouth Dockyard with baskets. It was the custom of his fellow workmen for one of them occasionally to read aloud one of the journals, or magazines. This, with their conversations, gave him an ardent desire to visit objects and places, with which he had thus been made acquainted; and at the age of 28, he travelled on foot, and alone, to Exeter, a distance of 42 miles, and this not by the usual highway, but across Dartmoor, because it is more sequestered; and he had once before been about 20 miles on the high road. He remained at Exeter about ten days, to obtain a knowledge of the localities, and the geographical relations of the streets, which he still retains, as well as of the various roads between that city and Plymouth. Returning to Plymouth he resumed his occupation, and, during the several years of his residence there, he made frequent excursions in and around the town, to places distant five, seven, or nine miles. Whenever any discovery was made in the neighbouring quarries, or any singular occurrence took place, he rarely failed to become acquainted, by personal observation, with the various localities connected with the circumstances. The alterations in the streets, &c., he used to visit and examine at night, when all was still; and thus he has acquired a better knowledge of his native town than many who have not lost their sight. He was in the constant habit of walking across the Hoe, an extensive natural esplanade between the town and the sea, to bathe; yet no accident ever befel him; nor, though obliged to scramble among the rocks, did he miss his way, or fail to return to the spot where he had left his clothes. By this constant exercise of his powers he augmented at once the energy of his faculty of locality, and acquired experience of the influence which air, and the various objects around him, or

with which he came in contact, exerted upon his perceptive faculties generally. With his augmented powers, and the confidence inspired by these excursions, his desire of exploring increased, and he obtained a passage to Liverpool by HM. frigate *Lavinia*, in July, 1835. He proceeded from thence to Dublin; soon returned to Liverpool; and afterwards travelled mostly on foot to York, Hull, and through the midland counties to his home, having been absent about twelve months, and travelled nearly 600 miles. He came to London by a steamer a few weeks ago, and has since been busy in learning the various localities of the metropolis. He can already find his way to and from distant points, with an almost incredible facility. The noise and bustle of the crowded streets disturbs him, and he therefore prefers walking at night. Like most persons in a strange place, he asks his way, and, if an accurate description be given to him he rarely fails to find it readily. His sense of touch, both in the feet and hands is singularly acute, so that the different impressions conveyed by the ground enable him to ascertain places of safety or danger; even on a paved footway he can easily determine his approach to the curb. Large and elevated buildings, gateways, openings of streets, and squares, are all appreciable by him; and his recollection of such places enables him to find his way afterwards without inquiry.

The individual was present at the meeting this evening, and appeared to be a very intelligent though an uneducated man. In answer to questions proposed to him by members, he stated that he was unable to distinguish colours, and was satisfied that the cases which had fallen under his own notice of blind persons who had professed to have such a power, were deceptions.

The size of his *organ of locality* was *very great*.

T. Kitchener, Esq., was elected an ordinary member of the Society.

BRITISH MEDICAL ASSOCIATION.

Tuesday, January 16, 1838.

The following gentlemen were admitted members of the Association at the meeting of Council this evening:—

Joshua Ingham Ikin, Esq., Fountain-street, Leeds.
Dr. Green.
Mr. Nathaniel Esdaile, Sloane-street.
Mr. W. H. Hodding, Gloucester-place, Portman-square.
Mr. Wallis, Hackney-road.

The letter alluded to in last week's Lancet, as containing some queries relative to the objects of the contemplated National Faculty of Medicine, was read; also the President's answer explanatory of the views of the Association.

Mr. Lavies, of King-street, Westminster, was elected a member of the Council by ballot.

D. Davies, Esq., the Honorary Solicitor, gave a brief account of the proceedings of the Committee appointed to watch over the interests of the medical profession, affected by the Imprisonment for Debt Bill. The Committee had waited on several noble lords; and their views had met with a very favourable reception in almost every quarter.

Certain points of the Poor-law Question, and particularly the basis on which the salaries of the medical officers should be calculated, were then discussed.

Dr. Erdmann relates a case in which the external lining membrane of a portion of the intestinal canal took upon it, and performed the functions of, the internal mucous membrane for several years.

According to Shall's experiments, the saliva contains a fixed alkali, and acts almost universally as an alkali upon test paper.

Dr. Schneider, while injecting the uterus of a woman, who died during the fifth month of pregnancy, found that the injection passed on into the veins of the placenta; he therefore ranges himself on the side of those who advocate the existence of a direct communication between the vessels of the uterus and the placenta.

TO CORRESPONDENTS.

If our correspondent, *T. W. H.*, were to commence practising as a general practitioner, he would certainly be liable to prosecution by the Apothecaries' Company. The best line of conduct which he can pursue is to act as an assistant, until time bring with it a change for the better.

We cannot insert the remarks of *T. J.* upon the Swansea case, although our pages are, of course, open to any of those originally connected with it.

The communications of *Messrs. Edmonds, Squire*, and several others, have been received.

The Resolution of the medical practitioners of Devonport and Stonehouse, on the subject of the Assurance Offices, shall appear in The Lancet of next week, together with the names of the subscribers. So, also, shall a notice of the petition against quackery, from Leeds.